IRE Transactions

ON MILITARY ELECTRONICS

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PROFESSIONAL GROUP ON MILITARY ELECTRONICS

INFORMATION FOR AUTHORS

The PGMIL Transactions is intended to bridge the gap between the various disciplines contributing to military electronics. Since this includes most of the branches of electronics, the military, and many fields which are associated with but not actually within the realm of electronics, it is essential that the papers published be of broad interest. The emphasis should be on readable, thought-provoking material that stimulates an attitude of open mindedness and curiosity.

Papers are solicited in the following general subject categories:

Military sciences—Military science fiction, famous battles involving electronics, basic problem areas of military electronics.

Technical survey—Tutorial technical papers on radar, communications, navigation, systems and operations research, etc.

Integrating papers—Integration of concepts common to several fields, as for example, wave phenomena, information theory, etc.

Physical sciences—Fundamentals of modern physics that may influence the future of military electronics.

Mathematical concepts—Applications and implications of modern mathematical methods.

Associated subjects—Survey of fields that are neither military nor electronic but which are important to the advancement of military electronics.

Manufacturing—Industrial and military problems of reliability, quality control, etc.

It is requested that each paper be submitted in duplicate. Standard IRE practice should be followed in preparation of the manuscript and illustrations. Papers should be sent to James Q. Brantley, Jr., or Donald R. Rhodes, PGMIL Editors, Cornell Aeronautical Laboratory, Buffalo 21, N. Y.

INVITATION TO MEMBERSHIP IN PGMIL

Members of the IRE may join the Professional Group on Military Electronics as active, voting members by payment of the annual assessment of $2.00. Nonmembers of the IRE who qualify may become nonvoting affiliates under the new IRE affiliate Plan by payment of an annual fee of $4.50 in addition to the assessment of the Group. All applications for membership affiliation should be addressed to the Chairman of the PGMIL Membership Committee, William M. Richardson, The Ramo-Wooldridge Corporation, 1300 Connecticut Ave., Washington 6, D. C., or to IRE Headquarters.

IRE TRANSACTIONS® ON MILITARY ELECTRONICS

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Editorial

Transactions of the Professional Group on Military Electronics

It is our lot today to live in the time of the Pistol and the Claw: the threatening pistol of strategic massive retaliation wielded by the "great adversaries," and the incisive tactical claw serving to deter small-scale aggression and the so-called little wars. These ponderous entities place heavy demands upon the muscles of industry and the superiority of the technological fields.

At the critical hour, the nation will be decisively dependent for survival and ultimate victory upon the reflexes of its electronic nervous system. We, in the broad field of military electronics, shoulder an enormous responsibility for the fitness and resiliency of this nervous system.

For efficient execution of this responsibility we require a high degree of mutual understanding and respect among military and electronic scientists and must establish low resistance communications channels between the principals. Electronics people must have an intimate understanding of the intended military utilization of their equipment, and military specialists must have a real appreciation for the capabilities and limitations imposed by the state of the art as it is now and as it may be in the future. To this end the IRE has formed the Professional Group on Military Electronics, which initiates its TRANSACTIONS with this issue.

The TRANSACTIONS' program is intended to provide an exchange of information among those concerned with the military electronics science. The broadness of the spectrum of people falling into this category presents a serious problem from a publication point of view and demands that material be presented with special care. In past years, much effort has been expended in the IRE to improve the readability of papers, and with good result. We intend to emphasize this quality to the point of making it a keynote for the PGMIL TRANSACTIONS. This *must* be done if the program is to achieve its purpose.

As pointed out in the Liebers' paper in this issue, *attitude* of scientific investigators is extremely important. Elimination of mental fences, by changes in attitude, have precipitated some of the most important scientific break-throughs. To stimulate free thought and perhaps batter down some of these fences, we expect to initiate at an early date a series of papers in the realm of future military electronics applications (perhaps better called "military science fiction") based upon sound scientific fundamentals. We expect such a series to be of wide interest, to be educational, and to raise some eyebrows.

To promote better understanding among engineers as to the role their equipment plays in the military application, we initiate in this issue a Famous Battle Series with Sir Robert Watson-Watt's paper. This series will treat important military encounters in which electronics, or the lack thereof, played a major part.

There will be an occasional paper in the fields of general science and mathematics, two areas from which the military electronics engineer cannot divorce himself. Dr. Gamow's paper is the first in this category. Future subjects may include game theory, linear programming, radar and navigation in nature, and similar subjects, but these will appear only occasionally.

We welcome broad technical papers surveying the scientific problem areas of military electronics (*e.g.*, communications, navigation, etc.) so long as they remain within the Charter of the PGMIL and do not transgress the PROCEEDINGS program. Also of utmost interest are papers which, though neither military nor electronic *per se*, bear heavily on both. For example, the human machine, semantics, reliability, quality control, operations research, etc. Dr. Morse's paper on the latter subject gives a substantial first step in this direction.

For future issues we will continue to invite papers where needed, but would welcome well-conceived contributed papers on any applicable subject. Ideas and constructive criticism from the membership are earnestly solicited. For those who might contribute papers, we offer the simple but meaningful words of Lord Byron, as follows:

"Dear Authors! Suit your topics to your strength, and ponder well your subject, and its length."

—*The Editors*

PGMIL's "Place in the Sun"

W. R. G. BAKER
Chairman, IRE Professional Groups Committee

It is indeed an honor to be invited to submit an article to the very first issue of the PGMIL Transactions. I have had a sort of "preview" of the list of articles to be included in this first issue and the editors are to be congratulated on their selections.

In order to place the ever-increasing importance of the PGMIL contribution in its proper perspective, let me retrace briefly the philosophy behind the Professional Group concept and show how it is integrated into the entire IRE organization.

The concept of Professional Groups was adopted in 1948, to provide more adequately for the professional needs of specialized groups within the framework of the IRE. It already had been demonstrated by the American Institute of Physics that, on the one hand, a large undifferentiated society covering a wide field of interest cannot satisfy all its members. But, on the other hand, several small societies in the same field are beset by excessive overhead and by resultant financial problems.

In adopting the Professional Group principle, the Institute of Radio Engineers was attempting to realize a workable compromise directly: the Professional Groups were to care for their own specialized professional needs while IRE Headquarters provided guidance, coordination, and generalized services.

This Professional Group concept has fitted military electronics perfectly. Previously there had been very little permanency or continuing effort in the broad area of military electronics. During wartime, many industries and engineers were engaged in military work for the duration.

With the cessation of hostilities, the engineers of the industry returned to their chosen peacetime pursuits. This happened after World War I and started after World War II. The war in Korea caused an abrupt change, and the military electronic business has continued to expand even after the termination of the Korean hostilities.

The result has been that many companies have set up departments or divisions specializing in some phase of military electronics. In other instances, companies have been established whose sole effort is in the application of electronics to weapons and weapons systems.

Some idea of the magnitude of the military electronics business can be obtained from the fact that in 1956 the production and sales volume of the electronics industry was nearly 6 billion dollars; of that total, military electronics sales accounted for nearly 50 per cent.

Electronics is playing a vital role in the complex weapons systems which will be decisive in preserving United States military strength and political freedom.

In the missile field alone, missile acceptances for the Air Force are likely to reach 35 per cent of Air Force spending for major items of procurement and production within three to four years.

Air Force Secretary Donald A. Quarles has illustrated the importance of electronics in our new airplanes. For example, he pointed out that one of our present-day interceptors, such as the F-89 Scorpion, uses enough tubes or transistors for 80 home radios. Replacing the World War II B-29 which had nine electronics systems, we now have the B-47 with a complex of 14 electronics systems and with more than a 300 per cent increase in the number of tubes and related circuitry.

This continuation of effort in the highly specialized field of military electronics has resulted in an ever-growing absorption of engineering and scientific manpower. That it is still on the increase can be confirmed by a glance at the newspapers and trade magazines.

While engineers engaged in military electronics use essentially the same principles that are the tools of all electronics engineers, the applications, the environment, and the degree of reliability are vastly different—that is, to a major extent they are highly specialized.

To provide a common meeting place for these engineers with a great mutuality of interest, it becomes the responsibility of the IRE to recognize a Professional Group on Military Electronics.

The PGMIL performs another function that may not be so evident but which is of major importance. As we know, there are many electronics engineers working within the military services. These engineers are entitled to join the Professional Group on Military Electronics, where they can meet with the engineers engaged in military work in civilian industry with whom they have so many mutual interests.

Today our engineers have as great a stake in the security of the United States as do our armed forces. And, as our weapons systems continue to increase in complexity, the work of our scientists and engineers becomes even more vital to our national defense. This is not to depreciate the importance of the infantryman or the artilleryman, but it must be recognized that the effectiveness of these men is constantly increasing because of electronic contributions to the art of warfare.

The PGMIL is to be congratulated for its role, not only in strengthening national defense, but also in helping to attain the objectives and goals of the IRE.

A Message From Your National Chairman, PGMIL

CHRISTIAN L. ENGELMAN
Captain, United States Navy, Retired

Our IRE Professional Group on Military Electronics is now eighteen months old and has matured to the extent of proudly recognizing 10 chapters and some 2500 members. Its future looks very bright and there is an attitude of "going places" among its membership.

Among the signs that forecast the future is the initiation of a series of National PGMIL Conventions to be held in the nation's capital each year. The first of these is scheduled for June 17–19, 1957, and the theme will be "Missiles and Electronics."

PGMIL is built on the solid foundations created by the need for a group that relates itself to military electronics systems that generally cut across many other groups. PGMIL is not directly concerned with specialities of primary interest to other groups, but, of course, will be happy to cooperate with them in providing a forum when military electronics is involved. We are definitely "under way" and must now settle down to the routines of building a service to our membership and the IRE. This is not an easy task and greatly depends on you, the individual member, and on the chapters to which you belong. I hope the officers and members of our chapters will become more and more active, for through them will develop the true strength of the professional group.

There are many sections without PGMIL chapters that have an abundance of interest in military electronics. You can provide the spark by accepting the position of chapter organizer. It is a simple task, and I would welcome an inquiry from you in this matter.

PGMIL is very pleased with its first issue of the TRANSACTIONS. We extend our appreciation for the excellent accomplishment of the editors and those who assisted them.

Electronics and National Defense

CLIFFORD C. FURNAS
Assistant Secretary of Defense for Research and Development

Electronics has found application in almost every phase of warfare. One can go further and state that modern weapons systems would not be possible without reliable electronic equipment. Since technology is always changing rapidly, the military functions of logistics, weapon control, intelligence, and command call for an ever-improving pattern of electronic aids in order to keep pace with the supersonic missiles and the destructiveness of atomic weapons.

The application of this electronic technology to every phase of warfare has demanded the best resources of our industry and universities. Better than one-half of our available professional engineers and scientists are contributing to this important work.

In peacetime there is no way to fully measure the success achieved in application of electronics to the military needs. The pace for introduction of new equipments is dictated by the threat of new weapons, rather than the commercial yardstick of customer satisfaction. Against this backdrop, it is most important that the electronics engineer keep in view the objective of designing equipment that is capable of performing its function under even the most adverse and unexpected conditions. While a trend toward complexity may be inevitable, we must never let complexity compromise reliability of performance. We always stand in danger of outsmarting ourselves by developing marvelous gadgets that do fantastic things *part* of the time. Whatever equipment we have must approach the ideal of performing *all* the time, or it is not suitable for military use.

The Professional Group on Military Electronics is to be commended for its efforts to provide a forum for the interchange of ideas and information in the area of defense electronics. It will furnish a new stimulus and an incentive to the engineer and scientists engaged in the National Defense effort and will be very valuable in arriving at adequate solutions to the ever-changing and uniquely difficult problems of military electronic systems.

Operations Research*

PHILIP M. MORSE†

Introduction

IN THE physical sciences—physics, chemistry, and many branches of engineering—one starts studying a phenomenon by picking some phase or aspect of it, by *observing* some part of its manifold behavior. According to folklore, Newton started his study of gravitation by watching the fall of an apple, not the breaking of the apple stem, not the apple's bounce as it hit, but its falling to the ground. Next, after observation, in the physical sciences one tries to form a quantitative hypothesis, a *mathematical model* of the aspect observed, which will duplicate quantitatively some of its behavior. If one has been clever, or lucky, in his choice of model, its mathematical framework will go beyond the observations, will predict what might happen in other circumstances. Newton's gravitational hypothesis, for example, his mathematical model of action at a distance, predicted the possible motions of a baseball, of a bullet, and of the moon.

Next, in the physical sciences, comes the phase of *controlled experiment*. One compares the predictions of the chosen mathematical model with what actually occurs. One modifies the phenomenon in specific ways, measures its reactions quantitatively, and compares the results with the model's predictions. If we have not been clever, or lucky, we may have to discard our model and start over; but if our choice was good, the experiments will enable us to improve and strengthen the model, to fix some of the constants in the equations, to add a few equations. After a continued alternation of model improving and further experiment, we can begin to call our mathematical model of a part of the phenomenon a *theory*, and we can say we are beginning to *understand* the phenomenon.

I don't wish to go into the deeper implications of the phrase "scientific understanding" or the philosophy of the relation between the theory and the phenomenon itself. In earlier times it was often implied that the theory *was* the phenomenon, in some fundamental way; now we are not so brash. We usually say the theory *represents* the phenomenon and we are not unduly upset to find that other models can also represent it more or less well. From a pragmatic point of view these questions of essence are less important than those of utility. For if we have a theory, a mathematical model which can predict quantitatively what will happen in various specific circumstances, we are in a position to *control* the phenomenon, to make it help us. Electromagnetic theory has little to say about what electricity *is*, if that question has any meaning; it does enable us to *control* electricity, so it runs motors and tv sets and electric furnaces, instead of leaving it uncontrolled, as it is in thunderstorms.

So the method of physical science implies the progress from observation to the creation of a mathematical model, to controlled experiments for checking the model and finally to a theory, which predicts quantitatively some aspect of a phenomenon and which thus enables us to control it. I don't wish to imply, of course, that this is the only scientific method in use. Progress has often been made in the life sciences and in the social sciences without the use of mathematical models. For example, Darwin's great synthesis of palaeontology, ecology, and genetics—it would be straining definitions to assert that mathematics was essential to any part of his reasoning—yet, there is no doubt his enunciation of evolution produced that deeper insight, that interlinking of facts, that firmer basis for further research, which is the essence of scientific understanding.

One class of human activity which seems to be amenable to the techniques of mathematical model making and quantitative experimentation is the repetitive group action which is often called an *operation*. A battalion of soldiers doing its assigned job, a squadron of planes on a mission, a running factory, or a sales organization is more than a collection of men and machines; it is an activity, a pattern of operation. These operations can be studied, their regularities can be determined and related to other operations; they can eventually be understood, and they then can be modified and improved effectively. Their repetitive nature, and the fact that they often involve and are conditioned by equipment, apparently makes these phases of human activity much more amenable to the quantitative analysis of physical science than most social and individual animate action.

The scientific study of operations, of the sort just described, is now called *operations research*. Though the term is new, this sort of research is not new. Taylor and his followers, with their time and motion studies, investigated a small part of the field; traffic engineers have been struggling with another part; systems engineering is closely related, and so on. Perhaps the most useful service the new term operations research has performed is to emphasize the essential unity of the whole field, to force the recognition of similarities in areas hitherto considered unconnected and to make apparent the broad utility of a number of research techniques and mathematical models.

There are many definitions of operations research, some so all-inclusive as to be meaningless. The subject is a new one, and its limits are not yet defined by general

* Presented before the International Institute of Statistics at Rio de Janeiro, Brazil, August, 1955.

† Massachusetts Institute of Technology, Cambridge, Mass.

usage. However, a review of the actions of operations research workers, not their claims, seems to show that operations research is the study of the operations of war and of peace by the use of the research techniques of physical science. It uses mathematical models and quantitative experiments to gain an understanding of these operations, in the scientific sense of the word understanding. The practical applications of the research are of use to the manager or officer directing the operations, in enabling him to exercise more effective control over it. As with other pure research, if the work is artfully and thoughtfully carried out, the resulting increase in understanding will have practical utility.

Any field of mathematics, any technique of measurement is used which will bring results. The theory of probability and statistics are useful mathematical tools; the experimental techniques of the psychologist are sometimes needed. But this does not mean that operations research is applied statistics on the one hand or is a branch of social psychology on the other. It uses any and all of these techniques to study *operations* so that they may be understood and thus understandingly controlled. Since a variety of techniques is needed, much of the research can best be carried on by a team of workers having a variety of basic research training, each contributing his specialized knowledge to the study of the operational problem. The advantage of the mixed team for the study of many problems is obvious. In fact, some persons have said that the use of mixed research teams is a characteristic of operations research. It certainly is important in many investigations; whether it is characteristic or necessary might be questioned.

Queueing Theory

Automobile traffic should be a good subject for operations research, but up till now little has been done, partly because of the complexity of the problem. Of course a tremendous amount of statistical data has been amassed and analyzed, but this is not operations research. A real application of the technique can only be made when some aspect of the problem is isolated in a way which makes it possible to form a mathematical model of its behavior. For example, we can choose to concentrate our study at first on single-lane, congested traffic, where each car must follow its predecessor, stopping when it stops and starting when it starts. This is a very special case, but perhaps if we can learn to understand its behavior, we can then see how to include more general traffic flow in our model.

There is a great deal to learn about this single-lane flow, as a matter of fact, much that is not yet understood. Its dynamics of stop-start are not simple. If the line is stopped, at the red light for instance, the cars are densely packed, bumper to bumper. When the light turns green, the line does not start as a unit; the first car starts, then the next, and then the next. A *wave-front of starting* travels down the line. Fragmentary observation indicates that the wave velocity of this starting wave is fairly constant, being about 25 miles per hour. In other words, if a car were sent in the opposite direction at 25 miles per hour, passing the first car just as it started, it would pass each following car in the line more or less when it started. The magnitude of this starting wave velocity depends on human reaction time and on the properties of automobiles. Further measurements should be made to see whether or not it is the same for lines of trucks as for pleasure cars and whether other factors make it change.

On the basis of such observations, one can then devise a mathematical model of the traffic line and see how the model will behave under other conditions. What happens, for example, when the line is moving and one car suddenly stops? Does a traffic light let the same average number of cars through per hour when the cycle is green for thirty seconds—red for thirty seconds, as it does when the cycle is green for two minutes—red for two minutes? If it does not, what is the cycle which offers the least average delay to the single-line traffic?

The single-lane, congested traffic problem may be a very small and special part of the whole subject of traffic, but you can already see that it is not simple and that a thorough, *quantitative* understanding of its behavior would help us quite a lot. In fact, it might suggest improvements in automobile design which would make the car a more effective part of traffic flow, rather than an individualistic, overpowered juke box. There has been as yet no systems engineering of automobilies at this level; it cannot start until more operations research is done on traffic flow, until models of the sort I have mentioned are developed and until operational experiments are devised to check out the models.

But let us concentrate our attention on another aspect of one-line traffic flow, even more specialized than the dense flow we have just speculated upon: take the case of a toll booth at the entrance to a toll highway. Cars come up to it, stop, pay the toll, and drive on. If a second car comes up right after the first, it must wait till the first car finishes, and if a bunch of cars come together, a waiting line, or queue, of cars will form and will last until all but the last have paid their toll, unless more cars arrive meanwhile. The length of time it takes to pay the toll varies from car to car, some have special tickets and hardly need to stop, others have no change and take quite a while—some may even stop to ask directions. For each specific toll booth we can measure the mean rate of flow of cars past the toll booth (S per minute, say) and also the probability distribution of the lengths of time each spends paying toll (the average length of time equals $1/S$, of course). We also can measure the average rate A of arrival of cars at the entrance and the probability distribution of the time intervals between arrivals. From these data we should be able to set up a mathematical model of the waiting line which should help us understand this very simple part of the traffic operation.

But why waste time or thought on such an elementary

part of the problem when there are so many really pressing traffic problems to be solved? The answer to that is perhaps the main point of the remainder of this article.

But to go ahead: we can, in this case, start making models before we make detailed measurements. For there are several limiting forms of the operation; any actual operation must turn out to lie intermediate between them. For any given rate S of toll booth service the toll collection might be completely regular, on the one hand, taking exactly $(1/S)$ minutes for each car, or else it might be completely irregular, taking a second for one car, 20 seconds for the next, and so on, so that S is only the average rate of service. To be specific, the probability distribution of the toll collection time may be the Poisson distribution, representing complete randomness. Likewise the arrival of cars may be very regular, spaced $(1/A)$ minutes apart, or their arrival may be random according to the Poisson distribution, or it may be something intermediate.

The case of both service and arrival entirely regular is trivial. If S is greater than A, *i.e.*, if the toll booth operator can collect the money faster than the cars arrive, there is never a waiting line, every car is served as soon as it arrives. If, on the contrary, A is greater than S, then the toll booth is swamped, and the waiting line continually grows longer until another booth is opened or until the cars get discouraged and turn back.

But a little recollection of toll booths should convince us that actual service and arrivals are far from regularly spaced in time. Probably the limiting case where both service and arrivals are purely random will be nearer reality. At any rate, it is a case for which we can set up a model. Here, of course, there will be fluctuations in the length of waiting line; after a bunch of cars arrive, it will be long; after a lull it will go to zero. All we can predict will be the average length of the line and the average delay. Here, because of the randomness, queues will occur even when S, the average service rate, is greater than A, the average arrival rate.

Suppose we call the probability that there be n cars in the waiting line P_n. We must set up equations relating the P_n's; these are obtained by equating the rates of increase and decrease of the lines. For example, the mean rate of production of a line of zero length is the mean rate of service S times the probability P_1 that there is only one in line—for when that one car starts paying its toll, there will be none waiting. The mean rate of "destruction" of a line of zero length is the mean rate A of arrival of a car, times the probability P_0 that it will find no car waiting (*i.e.*, at the most a car already engaged in paying toll). If A and S do not change with time, then a line of zero length should appear as often as it disappears, so SP_1 should equal AP_0. Consideration of the line of n cars is somewhat more complicated, for such a line can be created from a line of length $n+1$ by having a car move in front of the toll booth or from a line of length $n-1$ by having a car arrive. The equation of balance is $(S+A)P_n = SP_{n+1} + AP_{n-1}$.

This set of equations can be solved to obtain $P_n = P_0(A/S)^n$ and since the sum of all P_n's must equal unity (the line must be some length) we have $(1/P_0) = 1 + (A/S) + (A/S)^2 + \cdots$. If A is less than S, this series converges, and equals $S/(S-A)$. Therefore, the probability that just n cars will be in line is $P_n = (S-A)(A^n/S^{n+1})$, the mean length of the line is $L = P_1 + 2P_2 + 3P_3 + \cdots = A/(S-A)$, and the average time spent by each car waiting and then paying toll is $T = (1/S)P_0 + (2/S)P_1 + (3/S)P_2 + \cdots = 1/(S-A)$, as long as S is larger than A. If A is larger than S, the single toll booth will no longer handle the arrivals, the queue will grow longer continuously, and none of these formulas will apply. But the interesting and important cases are those for which S is larger than A, and waiting lines then only appear because service and arrivals are at random.

Here is a mathematical model. What can be done with it? We, of course, should investigate the other limiting cases of regular service and random arrival or random service and regular arrival. Or, even better, we should measure the statistics of the actual toll booth and then work out the model for it. But the other limiting cases are harder to work out, and it turns out that most toll booth operations are more nearly the random-random case we have just worked out than they are to any other limiting form. Moreover, this article is not supposed to be a complete treatise on the subject.

What the limiting case we have worked out predicts is that as long as service rate is less than about 50 per cent greater than arrival rate (A/S less than 0.7), the chance of encountering long queues is not great, and the average delay is only about twice as long as the average time taken to pay toll. But when arrival rate begins to get larger than 0.8 of the service rate, saturation effects set in, chances of finding short lines decrease, chances of finding long ones rapidly increase and average queue lengths and waiting times tend toward infinity. Further analysis of other cases, involving more regularity in arrivals or service, shows that a great deal of regularity must be present before predictions differ much from these worked out for the pure random case. For example, even if service is completely regular, if arrivals are random, the mean queue length has only reduced to half that given by the random-random formula; saturation effects and so on are still present.

But now to answer the question raised earlier, "Why bother with such a special case?" The original problem may be trivial, but the mathematical model is not trivial at all, for the model is applicable to many other operational situations besides toll booths. Waiting lines of customers form in stores and restaurants, ships wait in harbor for dock space, airplanes stack up over cloudy airports waiting for a chance to land, manufactured parts pile up in a production line waiting for the next step in assembly, long distance calls have to wait for a clear trunk-line, machinery breaks down and has to wait for the maintenance crew, and sales slips wait to be

entered into customers' accounts by a bookkeeper. In each of these cases the same model applies, in each case the formulas worked out here apply if there is fairly random arrival and service (or the appropriate modifications can be made for more regularity), and in each case the formulas can be used to predict the operational situation and to modify it for best results.

For example, we can predict the change in time spent waiting for dock space if there is an increase in ship traffic to a port, or we can balance the loss caused by the longer wait against the cost of more service facilities and arrive at a figure for the least costly balance between ships and docks, or between in-process inventory and production line, and so on.

This subject of waiting lines and their behavior is appropriately called waiting line or queueing theory. Many ramifications have by now been worked out: multiple service facilities, sequential lines, cases where the service rate changes with queue length, and so on. By now a variety of operational problems connected with waiting lines can be solved quantitatively, and many operational conditions confronting traffic engineers, production or merchandising experts can thus be understood and controlled or obviated in an optimum manner. Further theoretical work is needed, of course, for not all the possible combinations have yet been analyzed. Some of the analysis will require the full capabilities of competent applied mathematicians trained in probability theory; some of the calculations will need the full capacity of an electronic computing machine. But even now, queueing theory has already provided operations research with a mathematical model which is useful in many different operational situations.

Linear Programming

There are other mathematical techniques which also turn out to be applicable to a variety of operational problems. Let's start again with a very simple example, keeping in mind that other, more complex problems can be analyzed in a like manner. A factory makes diesel locomotives. At full capacity on one shift per day it can produce five locomotives per quarter year (it might be 50 or 500, but we are keeping the problem simple right now so we can see what goes on) and each locomotive costs five units to produce (the units might be $10,000 or whatever). If it has to produce more than five units per quarter, it can do so by adding another shift and by working some personnel overtime. However, only two more locomotives can be built per quarter by these added efforts, and these extra ones cost seven units apiece instead of five units. In addition, if more locomotives are built in any quarter than are sold, they have to be sent to a storage depot until sold, which costs one unit per locomotive for each quarter in storage. The sales manager for the company has just predicted that the sales for the next four quarters are going to be 2, 3, 5, and 10 locomotives respectively. How should the production manager plan his year's production so that sales requirements be met each quarter and costs be kept down?

Suppose x_n be the regular production in the nth quarter and y_n be the overtime production (then x_n cannot be larger than five and y_n cannot be larger than two). The total cost of producing the locomotives and storing those not sold each quarter is then

$$\begin{aligned} U = {} & P_r(x_1+y_1+x_2+y_2+x_3+y_3+x_4+y_4) \\ & +P_0(y_1+y_2+y_3+y_4)+W(x_1+y_1-2) \\ & +W(x_1+y_1+x_2+y_2-2-3) \\ & +W(x_1+y_1+x_2+y_2+x_3+y_3-2-3-5) \\ & +W(x_1+y_1+x_2+y_2+x_3+y_3+x_4+y_4-2-3-5-10) \end{aligned}$$

where P_r is the regular production cost $(=5)$, P_0 the overtime extra cost $(=7-5=2)$, and W the warehousing charge per quarter $(=1)$. In addition, the company policy is to plan to meet sales requirements without delay (*i.e.*, x_1+y_1 must be larger than 2, $x_1+y_1+x_2+y_2$ greater than $2+3$ etc.)

We could satisfy all requirements by having no overtime production, making five locomotives each quarter. But this would mean that three of the locomotives produced in the first quarter would be stored until the last quarter, costing three extra units apiece, which would cost more than if they were made later on overtime. Since all three can't be made in the last quarter, when they are sold, we can make two at overtime then and one at overtime in the third quarter. Therefore, the production schedule involving least cost is 2, 5, 6, 7, for successive quarters. Of course, if it is company policy to keep regular production as uniform as possible without additional cost, the schedule could be 3, 5, 5, 7.

This problem is simple enough to do by inspection. But what of similar problems, with thousands of units ordered, hundreds of different products made, other limitations on machine capacity and finite shelf life on some products? A solution by inspection soon becomes impossible, and some analytic or machine method has to be found.

Problems of this sort are called problems of *linear programming*. The problem is to find the appropriate values of a set of N variables x_n, subject to limitations of the sort $a_1x_1+a_2x_2+\cdots$ must be less than (or greater than) A, $b_1x_1+b_2x_2+\cdots$ less than B, and so on, and such that a certain cost function $U=u_1x_1+u_2x_2+\cdots$ be a minimum. The problem is called linear because all the functions involve the x's linearly (to the first power). It differs from the usual minimization problems because the minimum depends not so much on the functional form for U as on the nature of the limitations on the x's.

Parenthetically, the optimization of crude-oil-cracking production is a linear programming problem. An oil company can produce various proportions of fuel oil, lubricating oil, and gasoline from their cracking plants, depending on the crude used and on the cracking process used. But crudes differ in price, and cracking processes

differ in cost. If the company has orders for definite quantities of end products, what amounts of which crudes shall it buy and which processes shall it use in which cracking plants to produce the required products at least cost, subject to limitations of supply of crudes and outputs of their plants?

This is a straightforward linear programing problem, and some oil company engineers have been doing a good job solving it. These engineers did not call what they were doing operations research, some of them had not heard of the term. But neither were these engineers aware that many other problems in their own company's operations were also amenable to this same analysis. Their training and their outlook had been too specialized to see the wider possibilities. The value of the concept of operations research to this oil company lies in making the company research men aware that the mathematics they have been using can be applied to many more operational problems than they had hitherto conceived and in showing the company executives that they can use their own research departments to help solve production and sales and distribution problems, where they had not hitherto been used.

Theory of Optimum Distribution of Effort

I will mention just one more example of the unification of viewpoint which mathematical models can bring. It started, during the war, in connection with problems of naval operations, as a theory of search. Since the war its application in a number of industrial cases leads me to call it the theory of the *optimum distribution of effort.* Its naval aspect concerns the operation of search for an enemy vessel, or submarine or aircraft. The enemy is somewhere in a given area of the sea. How do you deploy your aircraft to find him? The connection between the mathematical model and the actual operation is the *rate of search.* A single plane can see the enemy vessel (by radar or sonar or visually as the case may be) R miles away, on the average. The plane can "sweep" out a band of width $2R$ as it moves along; the picture is analogous to a vacuum cleaner, of width $2R$, sweeping over the ocean at a rate equal to the speed of the plane and picking up whatever comes beneath it. An area equal to the speed of the plane times twice the mean range of detection will thus be swept in an hour. Sweep rates of planes vary from a few hundred square miles per hour to several thousand square miles per hour, depending on the plane, the radar equipment, and the vessel searched for.

If the enemy is equally likely to be anywhere within a certain area, then the problem is a straightforward, geometrical one. The search effort is evenly laid out over as much of the area as one has planes available. The problem is a little complicated by the fact that detection is not certain at extreme ranges, so the probability of detection falls off near the edge of the swept band, and there should be a certain amount of overlap between bands to improve the chance of detection near the edges.

But if the chance that the enemy is present varies from area to area, the problem becomes quite difficult, and nonmathematical intuition may lead to quite erroneous use of available effort. For example, if the enemy is twice as likely to be in one area than in another, then, if only a small amount of search effort is possible, all this effort should be spent in searching the more likely area and so on. A definite formula can be worked out in each specific case. Search plans for various contingencies were worked out by the operations research team attached to the Navy during the war; they materially aided the naval efforts in many cases.

Its seems a far cry from planes and ships and submarines to industry and business activities. But the utility of the mathematical models is their wide range of applicability. One possible business application of search theory comes in the problem of assignment of sales effort. Suppose a business has a limited number of salesmen, who are to cover a wide variety of dealers. Some of these dealers are large stores, which will usually produce large orders when visited; some are small stores with correspondingly smaller sales return. If there are enough salesmen, every dealer can be visited every month, and the optimum number of sales can be made, although the sales cost will be high. With fewer salesmen available, search theory indicates that the large stores should be visited. If the probable return per visit for each store is known, the optimum distribution of sales effort can then be calculated.

An interesting and typical variation on this problem comes when we consider the action of the individual salesmen, when we try to make their behavior conform to the best over-all distribution for the company. For each individual salesman, with his limited effort, it may be best *for him* to visit only the large stores; if his visits are uncontrolled and if he is paid a flat commission, it may turn out that the large stores are visited too often, the small stores too seldom, for best returns *for the company as a whole.* It then becomes necessary to work out a system of incentive commissions designed to induce the salesmen to spread their efforts more evenly between large and small customers. If the general theory has been worked out, this additional complication can be added without too much difficulty.

This problem of balancing the tendencies of different parts of a large organization is one which is often encountered in industrial operations research. The sales force is out to increase sales of all items, though sales on the other product are increasing; and the financial department frowns on building up large inventories, though small inventories always put the production division at the mercy of sales fluctuations. It is often not too difficult to suboptimize each of these divisions separately, so each is running smoothly and effectively

as far as its own part of the business is concerned. But to be sure that all these parts mesh together to make the company as a whole operate most efficiently requires much more subtle analysis and very careful quantitative balancing.

In the interest of reducing factory overtime and to keep down inventory, for example, it may be necessary to modify the salesman's incentive commissions, so he will be induced to push one line over another. It may be necessary for the production division to allow more overtime in one department than another, to make some part of its operation run at less than optimum in order that the over-all operation be optimum; and one must take care not to have a conflicting bonus policy which will penalize the production department for reducing its efficiency so that the effectiveness of the whole is improved. Solutions of company-wide problems of this sort require all of the techniques of operations research and then some. They are not ones for a newly-formed operations research team to try at first; but they are problems to head toward. If the team can be of even the smallest help to the top executive in solving such company-wide problems, they will have paid their way many-fold.

In tackling such general problems the operations research team is entering the field hitherto monopolized by management consultants. This should not be a subject of concern to either management consultants or to operations research workers. There should be very little overlap between the two, for the point of view and background of the operations research worker differs considerably from that of the usual management consultant. The management of a large company's operations is usually a complicated enough problem so that there is room for the help of both types of expert.

Sometimes these problems of balancing the requirements of various parts of an industrial operation to make the whole function optimally can be clarified if one thinks of the system as a servomechanism, with interactions and feedback circuits. For example, a factory, warehouse, and sales organization is a mechanism to produce flow from raw material to sales. Sales usually fluctuate in amount from month to month; the factory would like to keep its production uniform. The feedback from sales to factory (the system for reordering items in short supply) should be analogous to a low-pass filter, using the warehouse as a cushion so as to avoid sudden changes in production and yet satisfy immediately at least 90 per cent (for example) of possible sales demands. Once the circuit analog is worked out, it should be possible to calculate how much more it would cost to fill sales demands immediately in 95 per cent rather than 90 per cent of the cases, and so obtain a quantitative criterion for judging this sort of sales policy.

There are many other mathematical models which can be used to give us deeper understanding and control of various operations of peace and war. Game theory, developed by Von Neumann just before the war, has proved valuable in the study of military tactics, but so far has not had many useful applications to industrial operational problems. Present techniques of solution are predominantly for the case of two opposing sides, a situation more applicable to war than to peace (though some applications might be made to police operations against an organized racket, for example). If and when the theory can solve problems involving more than two competitors, then applications to business will be more fruitful. But some fundamental theoretical questions must be answered before further progress can occur in this field.

"Many present-day communication systems are extremely inefficient in that they fail to make use of the statistical properties of the information source. Suppose we are interested in a system to transmit English speech (no music or other sounds) and the quality requirements on reproduction are only that it be intelligible as to meaning. Personal accents, inflections, and the like can be lost in the process of transmission. In such a case we could, at least in principle, transmit by the following scheme. A device is constructed at the transmitter which prints the English text corresponding to the spoken words. This can be encoded into binary digits using, on the average, not more than two binary digits per letter or nine per word. Taking 100 words per minute as a reasonable rate of speaking, we obtain 15 bits per second as an estimate of the rate of producing information in English speech when intelligibility is the only fidelity requirement, a rate which could be transmitted over a channel with 20-db signal-to-noise ratio and a bandwidth of only 2.3 cps!"

CLAUDE E. SHANNON
"Recent Developments in Communication Theory"
Electronics, April, 1950

THE Great Discovery of Modern Mathematics*

LILLIAN R. LIEBER†

WITH DRAWINGS BY HUGH G. LIEBER†

This is not intended to be
free verse.
Writing each phrase on a separate line
facilitates rapid reading,
and everyone
is in a hurry
nowadays.

One of the amazing things
in the history of mathematics
happened at the beginning of
the 19th century.
As a result of it,
the floodgates of discovery
were opened wide,
and the flow of creative contributions
is still on the increase!
As E. T. Bell,[1]
the distinguished American mathematician,
says:
"The nineteenth century . . .
contributed to mathematical knowledge
about FIVE TIMES AS MUCH as was done
in the whole of preceding history."
And the rate of increase
has been even greater
since 1900!

Furthermore,
this amazing phenomenon
was due to a mere
CHANGE OF ATTITUDE!

Perhaps I should not say "mere,"
since the effect was so immense—
which only goes to show that
a CHANGE OF ATTITUDE
can be extremely significant,
and we might do well
to examine our ATTITUDES
toward many things, and people—
this might be most rewarding,
as it proved to be in
mathematics.

As we shall see.

Now what was the change in attitude
that led to this enormous increase
in mathematical productivity?

* Based on the article by Lillian R. Lieber, "Modern mathematics for scientists and engineers," *Trans. N. Y. Acad. Sci.*, ser. II, vol. 17, pp. 331–338; February, 1955. (By permission.)

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[1] E. T. Bell, "Men of Mathematics," Simon and Schuster, New York, N. Y., 1937, p. 17.

In order to understand this change clearly,
let us go back for a moment
to the work of Euclid,
with which we are all familiar.
As every schoolboy knows,
Euclid, in about 300 B.C.
gathered together the then-known
geometrical knowledge,
consisting of isolated theorems such as
the Pythagorean theorem and others
into a SYSTEM
that has been the model for
all scientific systems since then.
And what is the main characteristic
of such a system?
We all know that it is
the separating off
of some of the theorems
as "postulates" or "axioms,"
from which, by means of logic,
all the remaining theorems may be derived.

Now since
the "proof" of a theorem means
its derivation from previous theorems,
the first group of theorems (or postulates),
therefore,
cannot be proved,
since there is nothing preceding them
from which to derive them.
Thus,
any system MUST start with
UNPROVED theorems and,
by the same token,
also with UNDEFINED terms,
from which all the other theorems
will be proved
and all subsequent terms defined.
This observation always comes as a shock
to the layman who,
though he is unacquainted with mathematics,
still worships it at a distance,
believing that THERE, at least,
is a domain in which
one can prove EVERYTHING,
and which is therefore
absolutely reliable.
Euclid himself, however,
understood very well that
his system rested on
UNPROVED propositions and UNDEFINED terms.

Why, then, did he believe
in the reliability of his system?
What was his idea of the
NATURE of the postulates themselves?
How could they serve as a starting point
if they themselves could not be proved?
The answer for him was, of course,
that the postulates were "self-evident truths."
In other words,
any proposition is either

1) a "self-evident truth" or
2) something which must be proved,
i.e., derived from "self-evident truths"
or from propositions which have already
been derived from such "truths,"
as shown diagrammatically in Fig. 1.

Fig. 1.

Among the propositions which
Euclid listed as postulates

was the WELL-KNOWN "PARALLEL POSTULATE"
which may be stated thus:
"Through any given point which is
not on a given line,
one, and only one,
line can be drawn which is
parallel to the given line."
Though Euclid listed this proposition
among his postulates,
he did so reluctantly,
because it did not seem to him to be
so "self-evident."
Hence,
he set about to prove it
from his other postulates,
but without success.
Other mathematicians then
made further attempts to prove it, but,
over a period of many centuries
all these attempts met with failure!

In the early 19th century,
AFTER 2000 YEARS OF FAILURE,
it dawned on several mathematicians
at about the same time,
that the REASON for this failure was
a WRONG ATTITUDE toward
the NATURE of postulates.
That is to say,
mathematicians realized that
postulates should not be regarded as
"self-evident truths" at all,
but rather as mere
MAN-MADE ASSUMPTIONS.
Accordingly,
the diagram in Fig. 1 should be
replaced by the diagram shown in
Fig. 2.

Fig. 2.

Now does it really matter
what the philosophical attitude is
toward the nature of postulates?
Let us apply this question of attitude
to the problem of
the parallel postulate.
Since the parallel postulate is not proved,
and is to be regarded
not as a "self-evident truth"
from which there is no escape,
but rather
as a mere man-made assumption,
we might discard it altogether
and replace it by
an entirely different assumption,
namely:
"Through any given point which is
not on a given line,
TWO lines can be drawn which are
BOTH parallel to the given line."

FANTASTIC?

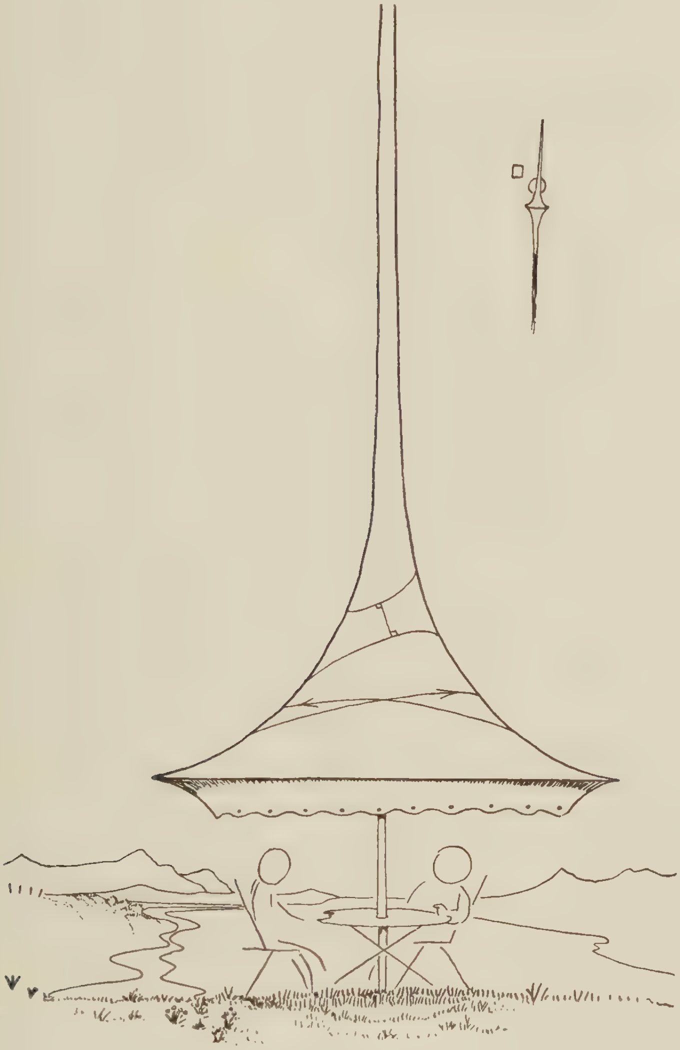
RIDICULOUS?

Not at all!

For, note that
the very concept of
a "man-made assumption"
gives us the FREEDOM
and the COURAGE
to CHANGE it and
follow through logically
to the consequences of
the new assumption.
This is exactly what was done
by the above-mentioned mathematicians
at the beginning of the 19th century—
namely, by
Lobachevsky, Bolyai, and Gauss,
all coming to the same conclusions
although working independently of one another
(as has so often happened in
the history of mathematics!).

And what was the result?
They succeeded in developing
a new kind of geometry,
a NON-EUCLIDEAN geometry in which
the parallel postulate was
DIFFERENT from Euclid's by
assuming TWO lines, instead of only one,
through a given point,
parallel to a given line (see above);
all the other Euclidean postulates
were retained.
This led to some
new unfamiliar theorems such as
"The angle-sum of a triangle
is LESS than 180°," etc. [1]
This very strange geometry
was later shown by Beltrami,
in 1868,
actually to apply on
a certain curved surface
(a "pseudosphere" [1])
rather than on the more familiar
"flat" surface of Euclid's geometry.

Not only that;
it is this non-Euclidean geometry
which applies to our physical universe
and was used by Einstein in
his *General Theory of Relativity!* [2]



Furthermore,
about 1850
the famous parallel postulate
was again replaced,
this time by Riemann,
who postulated:
"Through a given point
not on a given line
NO lines can be drawn
parallel to the given line."
This development led to
ANOTHER NON-EUCLIDEAN geometry
which was subsequently shown,
also by Beltrami,
actually to hold on a sphere,

on which the angle-sum of a triangle
is MORE than 180°, etc. [1].
THUS,
the view that postulates are
mere man-made assumptions and
are therefore subject to change
was fully justified.

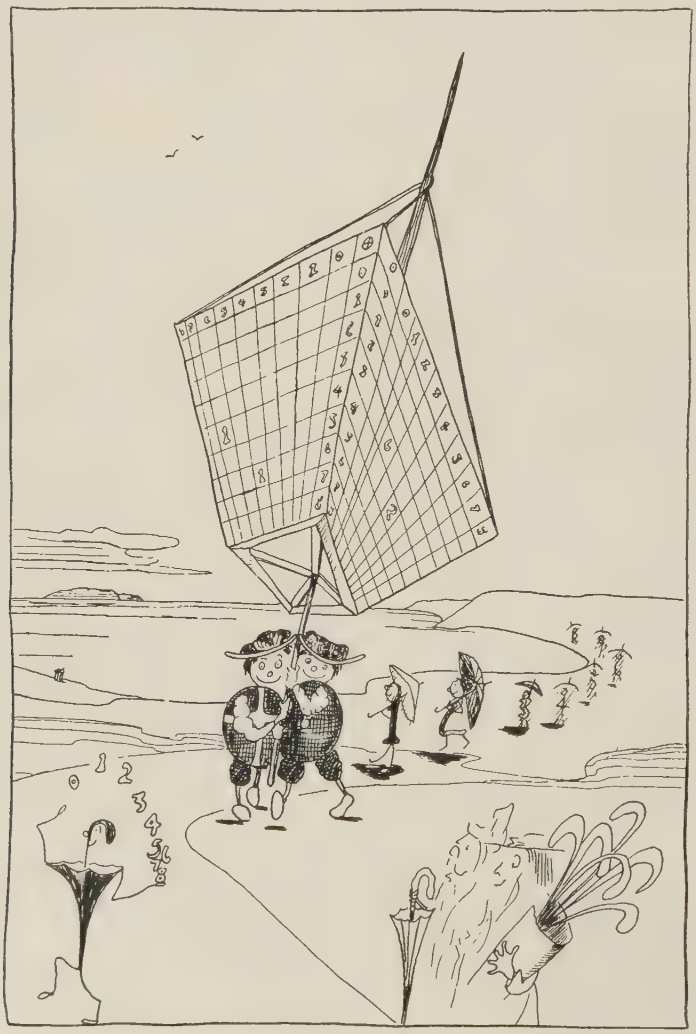
Indeed,
this was only the beginning—
for now mathematicians began to examine also
the postulates of ALGEBRA,
to CHANGE them,
and thus to achieve other
NEW algebras
(among them Boolean algebra, about 1850).
In fact,
a whole flood of
new mathematical systems
was inaugurated,
many of which already have had
useful applications,

and many more such systems will
undoubtedly be applicable in the future [3].

One use for such systems is
to prove the "independence" of
a given postulate in a system.
Thus,
even if the change in Euclid's system
had not turned out to be essential in
the study of the physical universe
(*e.g.*, by Einstein, as already cited),
it would still have been useful in that
it showed Euclid's parallel postulate to be
"independent" of the other Euclidean postulates.

In other words,
this parallel postulate is
NECESSARILY UNDERIVABLE from
these other postulates,
since its contradictories
(the one allowing two parallels and
the one allowing no parallels at all—
see above)
can also exist in harmony with
those other postulates,
as in the non-Euclidean geometries mentioned.
No wonder
the mathematicians could not prove
Euclid's parallel postulate—
even though they had the colossal patience
to keep trying for 2000 years!

Similarly,
Huntington [4] uses this idea
to prove the independence of
the postulates of ordinary algebra,
i.e., the algebra of complex numbers.
Thus he lists
27 postulates for this algebra,
and then proceeds to make up
27 different "algebras,"
each one differing only
with respect to one postulate
from his original set,
thus proving their independence,
as in the story of
the parallel postulate told above.



Now that we appreciate
the possibility of creating
VARIOUS mathematical systems—
and this is the GREAT DISCOVERY of
the early 19th century
from which the other discoveries flowed,
including geometries,
algebras,
arithmetics, etc.—
even logics, in the plural!—
let us turn our attention to
one of the NEW ALGEBRAS,
Boolean algebra,
and to some of its important applications.

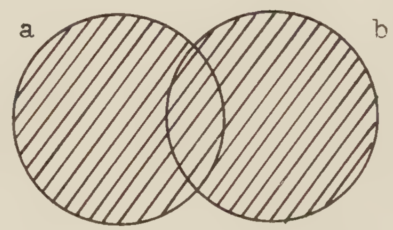
First of all,
in the spirit of
"postulational thinking"
discussed above,
the reader, of course, will want to see
a postulate set for
Boolean algebra [5]:

1) "Closure" for addition
$a+b=c$,
i.e., adding two elements of the system
gives a result which is
itself an element of the system.
2) Closure for multiplication: $ab=c$.
3) $a+0=a$
4) $a\times 1=a$
5) $a+b=b+a$
6) $ab=ba$
7) $a(b+c)=ab+ac$
8) $a+(bc)=(a+b)(a+c)$
9) $a+a'=1$
10) $aa'=0$
11) There are AT LEAST TWO
DIFFERENT elements in the system

Now,
if we try these postulates on
ordinary algebra,
we shall find that some of them work
[such as 1) through 7)],
but some of them [like 8)]
do NOT work,
and others [like 9) and 10)]
are meaningless.
But
realizing that all of these postulates
are man-made assumptions,
we know, of course,
that we have the RIGHT to make them
and to derive theorems from them,
provided only that there are
NO CONTRADICTIONS in the system—
for contradiction is
the only sin in mathematics—
which definitely cuts out
lying
as a successful tool of thought!

Further,
if we define the elements
(namely a, b, c, 1, 0, a', etc.)
in certain ways,
we find that ALL these postulates work,
and we shall be able to
use this algebra for various purposes.

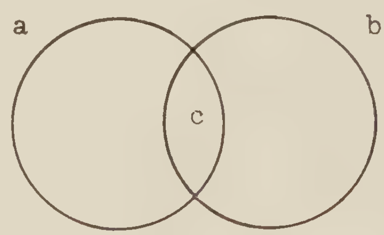
Let us first
take the elements to mean
"classes" of objects,
which we shall represent by circles
called "Venn" diagrams.
Now the SUM of two classes,
a and b,
is defined by
the shaded area in Fig. 3,



$a + b$

Fig. 3.

and the PRODUCT of two classes
is that part which
they have in common (Fig. 4).
Here $ab=c$.



$ab = c$

Fig. 4.

Let me illustrate this
"algebra of classes," Boolean algebra,
with a simple homely example.

Suppose a widower with one child
marries a widow who
also has one child,

and then they beget another child
by the new marriage.
Now represent diagrammatically
(by a "Venn" diagram)
the class of HIS children,
the class of HER children,
and the class of THEIR children,
as in Fig. 5.

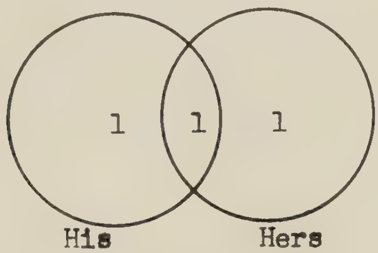


Fig. 5.

Here we can easily see that
2+2=3 (NOT 4, you notice)
and
2×2=1 (!)

(Remember the definitions of
SUM and PRODUCT of CLASSES given above.)

What then becomes of
the "eternal verity" that 2 and 2
always make 4?
We see that
in mathematics,
"eternal verity" has been replaced
by the more fertile idea of
"freedom to adjust"
for SURVIVAL and GROWTH.
But of course
this new-found freedom in mathematics
is under strict control
and is not just wanton license.

An application of Boolean algebra
to a very "practical" problem
has been made by F. E. Hohn,
in the Bell Telephone Laboratories [6],
to electrical circuits
in the following manner:

Let *a*, *b*, *c*, etc., represent switches;
let 1 represent
the condition of a switch being "closed,"
and 0 the condition of
a switch being "open."
Further,
let the SUM of two switches mean
connecting them in parallel,
as in Fig. 6,

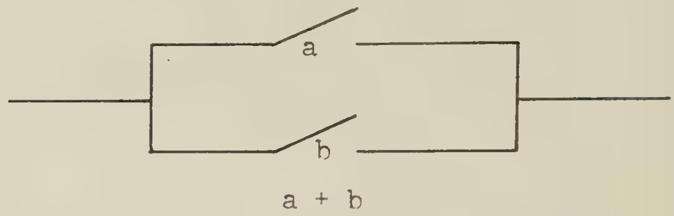


Fig. 6.

and let the PRODUCT *ab* mean
connecting them in series,
as in Fig. 7.

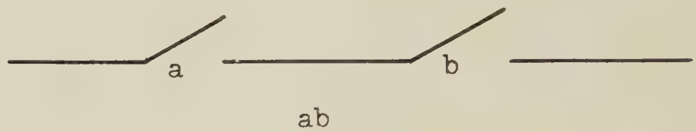


Fig. 7.

Since any switch can be only
"open" or "closed,"
we can have only
the following possible combinations
for a pair of switches:

SUMS
0+0=0
1+0=1
0+1=1
1+1=1

PRODUCTS
0×0=0
1×0=0
0×1=0
1×1=1

which means, of course, that
when the switches are
connected in parallel (sum),
then the entire circuit ($a+b$)
is "open" (=0) only if
BOTH switches are "open" (=0)!
and when the switches are
connected in series (product),
then the circuit is "open" (=0),
that is, the current cannot flow,
in all cases, EXCEPT
when BOTH switches are "closed" (=1).
Both tables given above,
it should be noticed,

are in agreement with
the postulates for Boolean algebra (p. 15).[2]

Finally,
a' is defined as shown in
Fig. 8.

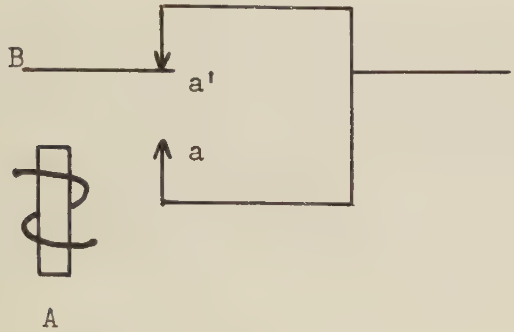


Fig. 8.

Here A is an electromagnet which,
when activated,
attracts the spring B,
thus making an electrical contact at a
and breaking it at a',
and when A is not magnetized,
spring B jumps back and
makes contact at a'
and breaks it at a.

If we now have an electrical circuit
as shown in Fig. 9

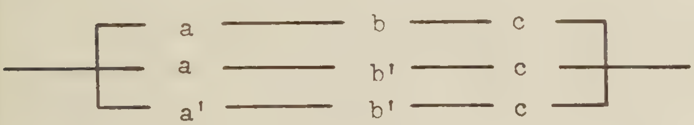


Fig. 9.

with nine contacts,
we may represent it algebraically
(in Boolean algebra)
by the function:
$f=abc+ab'c+a'b'c$
(See Hohn's definitions of
SUM and PRODUCT
cited above
and apply them to the diagram
in Fig. 9.)

[2] Some of them follow directly from
the postulates themselves,
and the others can be
easily proved from them.

We may transform this function as follows:

$f=c(ab+ab'+a'b')$ by postulate 7
$=c[a(b+b')+a'b']$ by postulate 7
$=c[a(1)+a'b']$ by postulate 9
$=c(a+a'b')$ by postulate 4
$=c(a+a')(a+b')$ by postulate 8
$=c(1)(a+b')$ by postulate 9
$=c(a+b')$ by postulate 4

which is, of course,
equivalent to the given function
and will produce
the same desired effect.
If we translate this
back into a diagram,
we have the situation shown in Fig. 10

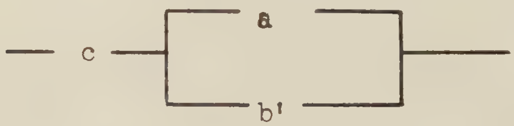


Fig. 10.

with only THREE contacts,
thus resulting in a large economy of "hardware"
without loss of effect.

Another very useful application—
one entirely different from the above—
is to logic itself,
but we cannot go into it here [5].
I shall merely say that,
by means of this algebra,
Aristotle's treatment of categorical syllogisms
has been reduced to
a mere three lines,
and the additional
hypothetical and disjunctive syllogisms
have been reduced to two more lines!
Thus,
all of "traditional logic,"
developed over the centuries
and occupying tomes of literature,
has been condensed to
a mere FIVE lines
by means of this
wonderful modern tool of
Boolean algebra!

I hope this brief account of
modern mathematics
has made clear, to some extent,
how very important have been
the changes in mathematics
since 1800,
and how necessary it is

for many people to
become acquainted with these new methods
by helping to introduce various courses,
such as non-Euclidean geometry,
Boolean algebra,
and many others,
into the curriculum of study
in undergraduate schools,
and even in high schools.

And of course
the Great Discovery of modern mathematics—
namely,
the NEW ATTITUDE toward POSTULATES
which has revealed
the great FREEDOM and POWER of
the postulational method of thinking—
this Great Discovery
applies not only in mathematics,
but also in the SCIENCES,
both PURE and APPLIED,
and indeed
WHEREVER we have to THINK STRAIGHT!

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Famous Battles—I

Battle Scars of Military Electronics—The *Scharnhorst* Break-Through

SIR ROBERT WATSON-WATT†

This paper opens our Famous Battle Series with an account—from an electronic point of view—of the daring escape up the English Channel of the German ships *Scharnhorst, Gneisenau,* and *Prinz Eugen* during World War II. We hope to follow this in a later issue with an analysis from the German side.

—*The Editor*

MILITARY electronics owes a good deal to *Scharnhorst* and *Gneisenau.* Not so much, however, that the much harried refugees should have been accorded the electronic immunity which Winston Churchill alleges for them, in his account of an impudent and successful dash across Britannia's very doorstep; this in broad, if subdued, daylight.

That most precise of all radar aids to blind bombing, *Oboe,* would not have been developed so early but for the pardonable irritation felt by Britannia as she looked across the *Rue de la Manche* and saw those unheavenly twins of the occupation more or less comfortably installed, in more or less permanent residence, at Brest. Even *Oboe* did not carry any early or direct conviction to these alien tenants that they should repatriate themselves to the already impaired *Gemütlichkeit* of the *Heimatland* or try the winter sports of still more northerly waters. In the end they departed. *Oboe* found other targets against which it was spectacularly successful. We had good reason to be grateful to *Scharnhorst* and *Gneisenau* for encouraging its start in life.

Military electronics churlishly withheld the modest aid which *Scharnhorst* and *Gneisenau* sought. My friend, Captain Helmuth Giessler, the navigation officer of the *Scharnhorst* in February, 1942, has recently recorded[1] the nature of the help expected. "In addition to the normal navigational aids, radar range-finding was to be used for the first time. Two methods were available. By one the ships would determine the direction of radio transmitters ashore by using a kind of identification system (IFF). The range-finding apparatus on the ships was not switched on for this purpose, but the direction was determined with the receiver of the identification system. It was also arranged that the ships' direction would be found, and their distance measured, by the range-finding equipment placed at selected points ashore. The results would be communicated to the ships and also to the operational navy and air force staffs who could thus link in with the movements of the break-through. Both systems failed during the operation. The reason for this was, among other things, that because of the extreme secrecy imposed, the radar operators could not be told of the vital importance of their new jobs, and it was not possible to practice in advance. It should be mentioned here that, at this time, the German radio range-finding apparatus could only indicate the direction and distance of an object. The PPI displays in universal service today were not then known. It was therefore impossible to use radar for navigation in 1942 as it is used today as a matter of course."

In any event "Until first light, navigation was only possible by dead reckoning because the radio beacons were either not transmitting or their direction could not be fixed. Technical deficiencies seem to have been the reason. The bearings which had been expected from the radio range-finding apparatus ashore either came much too late or were in error when they reached the flagship." That human fallibility which we shall have to examine elsewhere in this story does not seem to have been a monopoly of either side, although some excuses, legitimately claimed by the Germans, were not admissible from the British.

In spite of these disappointments, and in spite of certain other discomforts and inconveniences, the twin battle cruisers, *Scharnhorst* and *Gneisenau*, with the heavy cruiser *Prinz Eugen*, and with a small armada of destroyers, mine sweepers and ancillaries, maintained an average speed of some twenty-eight knots from Brest, up the English Channel, through the Straits of Dover at high noon, and out into the North Sea, which thus, temporarily, resumed its lost title of The German Ocean.

The Operation was not merely expected by the British; their intelligent crystal ball had even thrown a

† Adalia, Ltd., Montreal, P.Q., Canada.

[1] *J. Inst. Navig.* (London), vol. 9, p. 378; October, 1956.

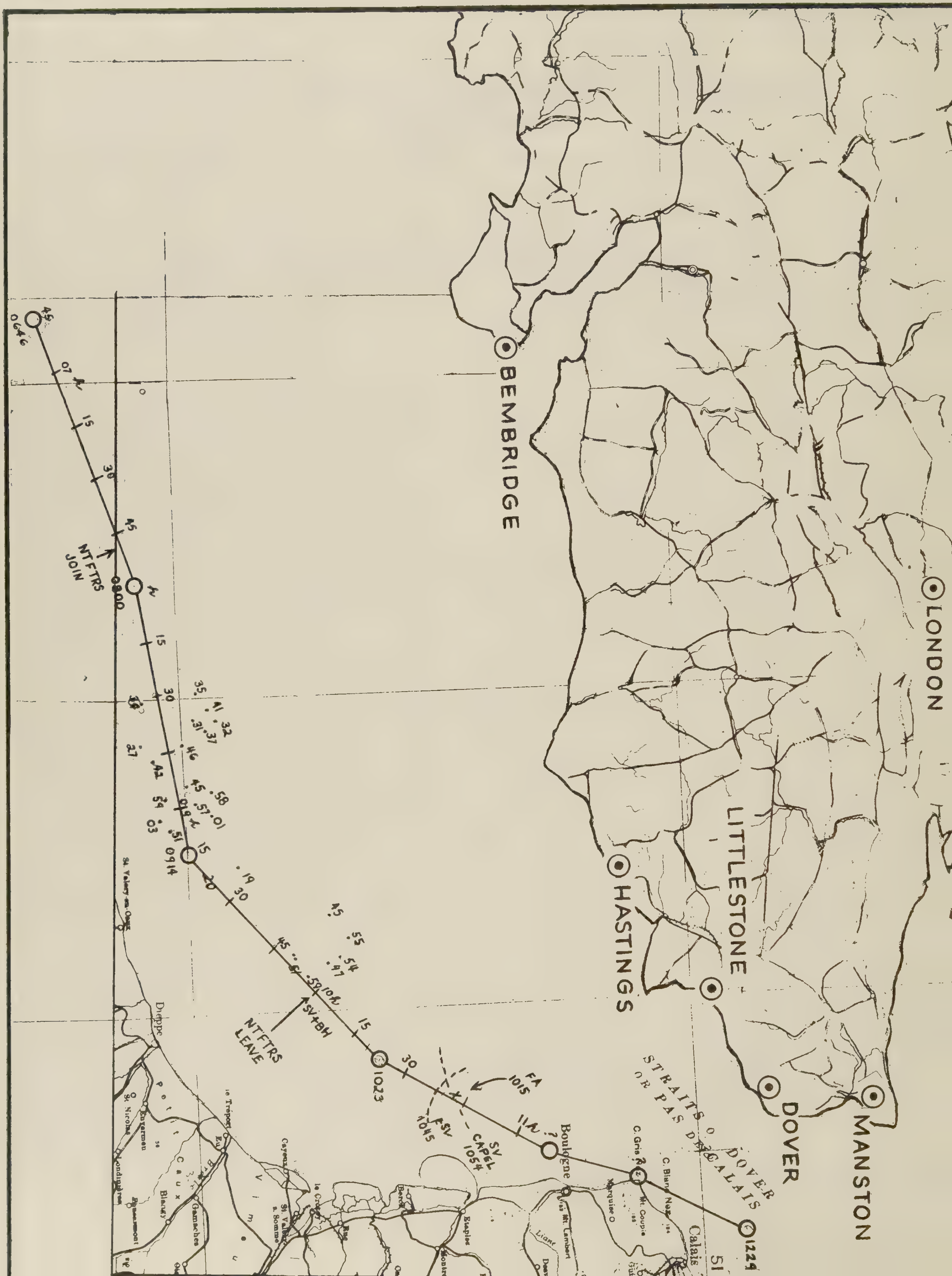

Chart showing check points along the route of the *Scharnhorst* up the English Channel.

spotlight on six days of the calendar sheet for February, 1942. Their radar chains had done their indispensable part in winning the Battle of Britain, their airborne radar was making life very uncomfortable for the U-boats, the big guns at Dover were, with radar aid, already making the port of Boulogne, slant wise across the Straits, a risky place to enter or leave, with or without air escort, in convoy, or in independent sailings.

Prime Minister Churchill has not concealed his chagrin—shared by every Briton—that a new Admiral Tromp should have worn an almost visible broom at his masthead as he swept majestically by the mouth of London's own river. Lord Rutherford, when faced by the perplexities of cooperative scientific endeavor, was sometimes heard to sigh, "There's too much human nature about." One can almost hear a Churchillian emendation, "There's too much inhuman hardware about" through his account of February 12, 1942, though what he wrote was " . . . when the enemy ships were spotted, the radar of our patrolling craft broke down. Our shore radar also failed to detect them. At the time we thought this an unlucky accident. We have learned since the war that General Martini, the Chief of the German radar, had made a careful plan . . . the jamming had grown so strong that our sea watching radar was in fact useless. . . . To allay complaints an official inquiry was held which reported the publishable facts."

I have an extremely high regard for the professional competence of my now very dear friend Wolfgang Martini. But I have my own professional pride! So I have fought a retrospective battle with the publishable facts. I believe military electronics can hold up its head again, praying the while for more effective countermeasures against the predictable and unpredictable interventions of human nature.

On February 8, 1942, Sir Philip Joubert, Air Officer Commanding-in-Chief, Coastal Command, Royal Air Force, wrote an appreciation which embodied high naval, as well as high air, opinion. The appreciation was communicated to the Air Officers Commanding his several Groups, and to Fighter Command, which held the responsibility for the radar system for early warning of air activity, as well as having also the responsibility for giving protective cover to air operations of other Commands. He wrote, *inter alia*, "During the past few days all three of the big ships have been carrying out exercises in the open water and they should be reasonably ready for sea. . . . As from the 10th the general conditions in the Channel would be reasonably favorable for an attempted break through in darkness. On the 15th February there will be no moon and the tidal conditions at Dover would favour a passage between 0400 and 0600 hours. The large number of destroyers and small TBs that have been concentrated at Brest would seem to indicate an attempt to force a way up the Channel. . . . There now appears to be a very strong indication that a plan to break through up Channel, with the object of returning to German home ports, will be adopted and is likely to be put into execution at any time after Tuesday, 10th February." No crystal ball can have been more transparently lucent and more clairvoyant.

What did we all do about it? (I use the "we" in a general and not in a particular sense, for at the material times I was in North America, talking radar and operational research to the U.S. Defense Establishment.)

Already on February 3, a quite elaborate system of reconnaissance patrols had been instituted for the specific purpose of watching for the first symptoms of a break-through. Three patrol lines were established, called *Stopper*, *Line SE* and *Habo*. *Stopper* lay across the bottleneck of the entrance to Brest harbor; *Line South East* ran from Ushant to the Channel Islands; *Habo* was short for "Havre Boulogne." All three patrols were flown by Hudsons fitted with ASV Mark II, a 200 mc/sec "Air-to-Surface-Vessel" search radar capable of detecting the presence of large surface vessels at ranges up to 30 miles.

The first aircraft for *Stopper* of February 11–12 flew off from its base in S.E. England at 6:27 P.M. met a Ju 88 en route, discreetly took evasive action, switched off its ASV, switched it on again at 7:20 P.M. (the patrol was to begin at 7:40)—and found it unserviceable. So it returned to base, the crew transshipped to another Hudson, and reached the patrol line at 10:38 P.M. *Stopper* began three hours late on RAF schedule. How late on German Navy schedule we shall see later.

Line SE was to be patrolled from the same starting time of 7:40 P.M. and to end at 11:40 P.M. "The aircraft detailed for the patrol reached its starting point at 7:36 P.M. but almost immediately the ASV equipment appeared to become unserviceable. The fault was of an obscure and unusual character which is still [I quote from a report dated March 2, 1942] under investigation. Although the aircraft remained on patrol for a considerable period until the fault was plainly established it is clear that there was no effective reconnaissance over this period. The failure was reported at 9:13 P.M. and the aircraft was ordered to return. It was decided not to send a relief aircraft." *Habo* was flown from 1:12 A.M. to 3:35 A.M. and from 4:35 to 6:31 A.M., on February 12. Nothing noteworthy was seen by *Habo*.

ASV Mark II was by now no longer an infant in Coastal Command. As early as April, 1941, almost a year before the break-through, Coastal Command had 110 aircraft operational with ASV Mk II, 50 of them with long-range antennas for anti-U-boat work, 60 with the forward-looking antennas used in the Hudsons. A U-boat had been damaged in February, 1941 as a result of ASV Mk II location; in March, 1941, ASV Hudsons had detected *Scharnhorst* and *Gneisenau* by night; in May, 1941 ASV Mk II located *Bismarck*. The Denmark Straits were in effect closed by ASV against enemy surface sorties. By the end of 1941, 94 per cent of all night sightings of U-boats were due to ASV Mk II.

The document already quoted, "Report of the Board of Inquiry appointed to enquire into the circumstances in which the German Battle Cruisers *Scharnhorst* and *Gneisenau* and Cruiser *Prinz Eugen* proceeded from Brest to Germany on February 12, 1942, and on the operations undertaken to prevent this movement," includes a statement about ASV Mk II which was doubtless publishable but ought not to have been a fact. If indeed it was true—and I doubt it—that "In the best circumstances the reliability and efficiency of these instruments in their present state of development and application in Hudson aircraft cannot [in March, 1942] be assessed higher than approximately 50 per cent," then two conclusions—at least—follow. One, that in Coastal Command Squadrons a higher measure of Christian forbearance and resignation reigned than I had ever detected; another that the allocation of aircraft for a major operation, likely to be over—if it happened at all—in less than two weeks, should have been so made as to discount this 50 per cent fallibility. Christian resignation in a military organization falls into the category of "human failure." So I chalk up two strikes against human nature in this man vs machine contest for a booby prize.

The *Stopper* Hudson was clearly not manned by modern housewives. When the electric kettle will not boil they look first for a blown fuse; the aircraft's crew "failed to detect the fault" which was only "later shown to be a blown fuse." Even "Cicero's" vacuum-cleaning colleague in the British Embassy at Ankara went straight to the fuse box! Having spent a period, which I infer to have been about an hour, looking for more obscure effects, the crew appear to have returned to base without making such a signal as might have brought a relief aircraft to the patrol line before 10:38 P.M. It was known, as the Board of Inquiry reports, that "On any but bright moonlight nights, these patrols depended upon their ASV to detect the enemy ships." "It was a very dark night and, visual reconnaissance being impossible, reliance for the detection of the ships had to be placed on the ASV." And as *Royal Air Force 1939–1945, Vol. 1*, says "The night was intensely black—so black that the crew of the Hudson could barely see the wing tips of their aircraft."

The score mounts. This patrol had been "laid-on" afresh some days earlier, it had, however, been flown for some months prior to the appreciation of February 8. It was therefore manifestly and freshly important, it was known to be useless save for ASV (or enemy aid!). That equipment had been in use in the Command for much longer than a year, yet crew training was clearly deficient, demands for improved serviceability had not been made sufficiently pressing, no provision for overlapping flights to give continuity of patrol had been made, instructions as to prompt action on ASV failure had not been given or had not been obeyed, initiative in breaking wireless silence, in conditions where the enemy could not hope to make a successful interception, was not exercised. There may be publishable explanations, but they have not been published. We had given them my "third best to be going on with"—but it was, in their circumstances, the supreme best, it was "ASV or nothing." Whether the Operational Research Section, Coastal Command, had analyzed a year's general, or seven days' particular, experience of ASV, whether TRE had been beaten firmly on the head, we do not know. "The rest is silence."

The *Stopper* having lost its magic bottle, *Line SE* remained a reasonable hope. I find difficulty in believing in another ASV fault so "obscure and unusual" that it was not explained within twenty days. I cannot reconcile myself to a belief that TRE, Operational Research Section, Coastal Command and Signals Branch, Coastal Command, could not have made a publishable diagnosis before March 2. Even so, the lost hour-and-a-half do not call even for Operational Research. It was inescapably the product of human error on the ground; the failure to give such instruction, and such instructions, as would ensure that the sole hope did not become a forlorn hope. This it did through confusion about the right action to be taken by aircrew in a too, too probable ("approximately 50 per cent"??) contingency. This was the more unpardonable after a year's general experience, a week's particular experience in full-scale practice, and near the midpoint of five crucial days clearly identified in advance, by the Command's own Commander-in-Chief, as crucial.

Habo was the back-stop. Surely the best use of its aircraft and crews, immediate and reserve, was to make up for nonprovision of reserves to fill the *Stopper* or, more timely and practicable, *Line SE* gaps. Of course it would have been difficult, inconvenient, hard on crews, but—serviceable ASV the only hope, the crystal ball of February 8 not clouded over as was the Channel sky, photographic reconnaissance on the afternoon of February 11 showing all the trio out of dock, six destroyers in waiting. An inconsiderate enemy had left torpedo booms about the ships, but we could scarcely hope that he would give us a one-hundred-per cent-serviceable early warning!

The obscurity of the ASV fault on *Line SE* might be coupled with Sir Philip Joubert's statement, "There was a suspicion that the patrol's ASV had been jammed during the earlier part of the night," to point towards an unjustified conclusion. I am assured by another postwar friend, Dr. von Scholz, who was in charge of General Martini's jamming operations against our radar watch, that the most stringent prohibition had been put upon any jamming during the break-through, until after 10 A.M. (British time), and that this rule was rigidly obeyed in respect of 200 mc as well as of the other frequencies.

Before we look at the performance of the shore-based radar chains, it is convenient to have a mental picture of the actual times at which the cruisers passed some familiar reference points. I am indebted to Captain Giessler for a series of timed fixes made by "normal

navigational aids." From these points I made rough but reasoned interpolations. This I did before I risked being prejudiced by knowledge of the data produced by this British radar which was alleged to have been useless on "the day." *Scharnhorst* cast off at 9:30 P.M. British time; the other two big ships took station astern of her; within an hour they were making 27 knots. The group passed just west of Ushant about a quarter of an hour after midnight (I shall use British time throughout this story), just west of the Casquets at 5:05 A.M. off Cherbourg a few minutes after 6 A.M., north of Barfleur about 6:30, and 55 nautical miles (n.m.) west of Dieppe at 8 A.M.

By 9:15 A.M. *Scharnhorst* was 23 n.m. west by north of Dieppe, 30 n.m. W by S of Le Treport; at 10 A.M., 20 n.m. northwest of Le Treport and 27 n.m. WSW of Berck; 10:30 A.M., 15 n.m. and a shade north of west from Berck; 10:45 A.M., 13 n.m. W by S of Le Touquet; about 11:45, a few miles off Boulogne; and an hour later about 10 n.m. from Calais and 17 n.m. from Dover. The later parts of the track are not of direct importance to this particular story.

I have not, at present, sufficiently detailed information on *Stopper*, *Line SE*, and *Habo*, nor on the detailed timetable of the ships from 9:30 to justify an attempt at microanalysis of the probability that *Stopper*, resumed at 10:38 P.M., should have made an ASV sighting of the big ships. There is a *prima facie* case in favor of the probability; the short official history *Royal Air Force, 1939–45* touches on it, but is insufficiently detailed or analytical for our purposes. It does, however, add the interesting information that *Stopper* had been flown for seven months before the breakthrough.

Had *Line SE* been resumed with serviceable ASV at any time before 5 A.M. it could scarcely have failed to pick up the ships. *Habo* was not flown at any time which is of interest in this story.

Now we turn to the ground-based radars which are alleged to have "failed to detect them." At 8:24 A.M. on February 12 the Beachy Head station of the air-warning chain located hostile aircraft 25 miles to northward of Le Havre. It became clear that several small groups of aircraft, probably at about 3000 feet flying height, were involved, and plotting continued until 9:20 A.M. New indications were followed from 9:47 to 9:59. The relation of these plots to the position of *Scharnhorst*, as inferred by the interpolation process indicated above, is as under.

The first group of plots, 0825 to 0920, contained eleven points to port of the track, one dead on track and eight to starboard. The extreme departure to port was 7, the mean 4 n.m.; to starboard 8, the mean 3; the mean departure was 3.4 n.m. The plotters diagnosed the tracks as being produced by aircraft circling over surface ships; the forward speed of the ships they estimated at "about 25 mph." The plot falling dead on track was also dead on time, within one minute; three others were within a minute of being dead on the beam of the ship, the average lead or lag was three minutes or so. All this is impressively in agreement with General Galland's account of the night fighters' tactics; they joined the ships at about 7:50 A.M. and held mainly to the side nearer their enemy; *i.e.*, to port of the ships' tracks. Orders called for very low flying to avoid radar detection, but it may legitimately be assumed that night fighters could not venture "wave-hopping" round an 8 A.M. dawn of an overcast and somewhat foggy winter morning in the Channel.

The second group of plots from Beachy Head radar ran from 9:45 to 9:59. The seven plots all lay to port of the assumed track, two by less than a mile, two by fifteen miles, the mean about 8. All but two were abeam or slightly ahead of *Scharnhorst*, none by more than some twelve minutes steaming, the average two minutes. Galland reports that the night fighters left their charges about 10 A.M. and were replaced by day fighters. I have attempted to decide what speed I would, at the time, have inferred for the ships, in the period 0855–0950, from the groups of air plots. I would, I believe, have made it something appreciably over 25 knots rather than 25 land mph. The navigational data shows $27\frac{1}{2}$ knots.

The air reporting picture is, for our purposes completed by one other report of hostile aircraft. Fairlight reported aircraft 15 miles west of Le Touquet at 10:15 A.M., and continued plotting them till 11:40 A.M., making thirty-two separate tracks. These stations used standard miles in their unofficial translation from grid reference; the 10:15 plot falls dead on the ship's track but ahead by about 27 minutes in steaming time, or something under 15 n.m. in distance.

Beachy Head now reenters the picture with a surface vessel plot. The operator decided that plots observed at 10:14 and 10:16 A.M. were due to two separate groups of surface vessels, estimated at three ships each, 44 and 46 miles respectively from his station. As he was an aircraft observer he presumably used land miles, and this would put the mean of the two groups dead on *Scharnhorst's* track. His grid reference, however, fits with nautical miles, and puts his fix $2\frac{1}{2}$ n.m. to starboard of track. As for time, his first plot is 2 minutes behind estimated position of *Scharnhorst*.

Fairlight's Naval type 271 radar plotted surface vessels 27 n.m. southwesterly from Cap Gris Nez at 10:45; *Scharnhorst's* estimated track shows her at this distance at 10:40, though SSW rather than SW. Capel type 271 reported a plot, estimated 20+ surface vessels, at 35 nautical miles, at 10:54; ship plot cuts this range at 10:44. It should however be noted that *Scharnhorst's* mean "ground-speed"—as a chairborne-aviator-non-sailor like myself might be forgiven for calling it,—fell from an average of $29\frac{3}{4}$ knots, over the 9:14 A.M. to 10:23 A.M. run of 34 n.m., to an average of 22.7 knots to the next fix (time unreported) 23 n.m. further on. This discrepancy in timing will almost certainly be re-

duced when the time of the intermediate fix is ascertained, as Captain Giessler hopes to do.

The detailed story of the jamming, which was withheld until 9 A.M. and did not begin to be at all troublesome until 9:20 A.M. and after, will, I hope, be told by the German Jammer-in-Chief or his immediately responsible officer.[2] Types CH and 271 were able to plot with little impairment, CHL was seriously affected when looking across the Straits. Capel 271 went on plotting till 12:44 P.M., to a range of 52 standard miles, Fairlight's 32 tracks ran through to 11:40 A.M. Bembridge plotted hostile aircraft at 120 land miles after the jamming was well established; this may have been some of the fighters returning to base at Abbeville. The ships themselves were plotted from a position about 25 miles west of the estuary of the Somme at 10:15 A.M., clear through the Straits of Dover, only the latter part of this track falling exclusively to the centimetric Type 271.

Am I obstinately blind in failing to discern failure of the shore-based radar? If I am held to my reiterated statement that radar is not merely an equipment or a group of equipments, but a system, then the radar system did fail but the electronics held out; the men behind the electronics were lamentably far behind. The case against the humans now calls for review.

When Beachy Head made its 10:14–10:16 observation immediate attempts were made to report to the Naval Plotting Room at Dover. But the telephone line was unserviceable! Despite this Beachy Head CHL station passed the plots to Dover at 10:40.

When Beachy Head CHL made its 8:24 A.M. observation on "hostile aircraft circling over ships moving up Channel at about 25 mph," this was reported immediately to Headquarters, Fighter Command, through the early warning system filter and operations rooms, which were the nerve center of the British radar network. The Board of Enquiry reports "such plots, which are common in this area, have a variety of explanations. They may indicate the presence of aircraft circling over ships, individual enemy aircraft exercising or testing their guns, or enemy 'Air/Sea rescue' aircraft, or they may be due to other causes. These plots formed the subject of three conversations between the Duty Air Commodore, Fighter Command, and No. 11 (Fighter) Group, which took place at 0845, 0930 and 1000. At 1010 instructions were given for a further reconnaissance."

It is ironic that only the jamming operation, which was designed to paralyze the radar system, was effective as a not-very-early warning system! It was only when the reports of "interference," which were made from 0925 onward, became so persistent as to stimulate consultation with the Operational Research Section that the Duty Air Commodore, Fighter Command, decided to tell Controller, No. 11 (Fighter), group at 10:50 A.M. that something unusual was happening. No better testimonial to the wisdom of General Martini and his colleagues could have been imagined than this success of their rigid discipline in the withholding of jamming measures in the early stages of the break-through.

Even this belated response to the uncovenanted benefits of military electronics came too late to outpace the blind goddess of chance. Bored by the inactivity of what appeared to them to be an inadvertent armistice, the Station Commander at Kenley Fighter Base and his Wing Leader begged for, and obtained, permission to go looking for trouble. Taking off at 10:10 A.M., their two Spitfires had just come in sight of the French coast, through bad and deteriorating visibility, when they stumbled on a couple of Messerschmidt 109's. "The idea of picking up a stray Hun" had been fulfilled, and soon more richly. The chase led them over two big ships, with a screen of destroyers and an outer ring of E-boats —and under a dozen inhospitable fighters. Without scope to climb on to the enemy tail, the Spitfires dived, to be greeted by a hundred-and-one gun (Flak!) salute. The great secret of the possible break-through had been withheld by British and German alike from the two stray-Hunters, as from so many others who ought to have been told. They did, nevertheless, regard this assemblage as unusual, and dashed home to report. I have a vague feeling that if Horatio Nelson had been in a Spitfire he would have thrown the rigidly-imposed radio silence of such sorties to the winds. But the hunters touched down at 11:09 A.M. with the secret still, to that moment, hidden in their breasts. "By 11:25," as the Air History records "all naval and air authorities were aware that the enemy battle cruisers, under strong and surface escort, were entering the Straits of Dover."

There is incomplete agreement between Giessler and Galland about the time of the first artillery salute from Dover. Galland puts it at 12:16 P.M. British time. Giessler says "up to this point (10:45 British time) on February 12, the enemy had taken no countermeasures. One British aircraft was sighted, but it was shot down by some of our fighters. The force was now approaching the Straits of Dover. It was only then that we saw signs of the first hostile reaction—the Dover batteries opened fire. All the shots fell far to port. Then British high-speed motorboats tried an attack but they were repulsed by our own boats."

Galland mentions a radio report, from a British fighter about 10 A.M. British time, of a big German group with three heavy units, and in total some 20 war vessels, about 50 miles off the Somme estuary, making high speed for the Straits of Dover. The sending of this message I have not been able to confirm by inquiry. Nor can I learn of any credible source for a radio signal, alleged to have been sent out at 9:20 A.M. (the time when the jamming was experienced at the radar stations) recalling all British aircraft to base in anticipa-

[2] Editor's note: The author refers to an analysis of the role German countermeasures played in the operation, which we hope to obtain from General Martini, possibly for the next issue of The IRE TRANSACTIONS ON MILITARY ELECTRONICS.

tion of heavy air attacks by the enemy. As for the shooting down of a British fighter before 10:45 A.M., I can only surmise that this fighter was one of the two who were dispatched at 10:10 after "three conversations (on the radar indications) between the Duty Air Commodore, Fighter Command, and No. 11 (Fighter) Group." This *Jim Crow* reconnaissance mission took off "at 10:20 for a sweep from Boulogne to Fecamp. About 15 miles west of Le Touquet (the leader saw) some twenty to thirty vessels, which he took to be a convoy with an escort of five vessels. Northwest of this convoy he saw some nine E-boats." Regrettably obedient to the injunction for radio silence, he too reported only after touch-down about 10:50. Vice-Admiral, Dover, and 11 Group were informed; the shipping strike which had been ordered about 10 A.M. after an earlier *Jim Crow* had sighted E-boats starting southward from Boulogne, and another E-boat moving northward from Berck, was now cancelled and a larger strike operation ordered.

Both Spitfires had, in fact, and despite Galland, returned safely. The junior Spitfire pilot, a Sergeant, contributed an interesting appendix to his Squadron-Leader's "debriefing." The Sergeant had spotted in the "convoy" a vessel with a tripod mast and superstructure. Handed an album of German ship silhouettes, he picked out a capital ship! But again blind chance was allowed to win. The interrogation had been so long that the firm identification by the two stray-Hunters won, on this handicap. Again one must wonder whether the radio silence order should have been maintained in circumstances where it was of extreme importance to the enemy that the secret should be kept, to the British forces that it should be known quickly to all who must act, and when the Spitfires had been so warmly received by the convoy as to suggest that their presence had been discovered!

I am not unmindful of Samuel Eliot Morison's wise comment on the Pearl Harbor story, in his *History of United States Naval Operations in World War II*, "It is no part of this writer's purpose to set himself up as a one-man court of inquiry. . . ." As the first Arch-Boffin, however, I have the Boffins'[3] privilege of asking questions, and remaining unashamed and unchastened if they prove to be foolish questions. So I ask: Why were Admiralty so firmly convinced that, in an operation which involved more than twenty-four hours exposure to opposition, the enemy would risk early discovery in daylight to assure passage of the Narrows in dark? Why did AOC-in-C Coastal not ensure that his Group Commanders informed lower formations of the high probability of a break-through in the five or six favorable days? Why did not his Group commanders tighten up every important element in their operational machinery, when they, at least, had the remarkably clear appreciation before them? Why did not Fighter Command pass the special expectations down the whole chain of those responsible for radar early warning? Everyone in it had been trusted with the precious secrets of his own side, why should the enemy's secret be withheld from those who had so distinguished themselves in radar reporting? Why did those at the midlevels, interpreting situations from radar evidence, not back the judgment of their observers and put their own weight behind that judgment? Why had they not made their own estimate of the speed of the "convoy"—following the Beachy Head staff's lead—to discover that it was making 25 knots or more? How often had they observed convoys, under air escort, making a dashing twenty-two, twenty-five, or twenty-seven knots? Was such a convoy not a worthwhile target, if only as a full-scale exercise? Why . . . but let me end by asking again why a Command which had had ASV for $2\frac{1}{2}$ years, whose Commander-in-Chief at this very time was the man who, in mid-1936, had asked me and my radar research staff to consider airborne radar to aid ocean reconnaissance for surface vessels, who knew that the most primitive ASV had located a ship in August, 1937, had found *Rodney* and *Courageous* from a range of 5 miles on September 3, 1937, *Courageous* and *Southampton*, in fine weather, from 9 miles in September, 1937, who had been using ASV in its Squadrons since September, 1939, who knew what I have recited earlier about ASV Mk II, who knew better than any how often "it was ASV or nothing"—why did they tolerate 50 per cent serviceability—if it was so—and why did they fly several gappy patrols instead of a smaller number of gap-free patrols? I do not attempt to penetrate the mysteries of communication and cooperation between Navy and Air Force, but there are unanswered questions in that field, too.

I would not have it thought that this is a pointless raking up of long-settled though incompletely analyzed mud. History exists as a beginner's guide to an uneasy truce, to our partially peaceful co-existence with human nature. In that other uneasy truce, in which we shall have guided missiles with which to destroy whatever then remains of human nature, we shall have to remember from history the examples of human failure to make the best, or the worst, use of the tools of military electronics.

[3] Sir Robert Watson-Watt, "The natural history of the Boffin," PROC. IRE, vol. 41, p. 1699; December, 1953.

Will Science Come to an End?*

GEORGE GAMOW†

WE LIVE in an era of tremendous advances of science and technology, and almost every month brings some exciting new development. The progress of scientific research is accelerating from year to year, and the number of persons involved in scientific inquiry increases correspondingly. Our big telescopes search into the far corners of the stellar universe, bringing us invaluable information about its present and its past. Our giant multibillion-volt atom smashers help us solve the secrets of the elementary particles from which all material bodies are made, and advanced methods of modern biological studies bring us to the verge of solving the mystery of all mysteries: the nature of life.

It seems, in fact, that in all major fields of science we come closer and closer to a complete understanding of the most deeply hidden secrets of nature, and that, with a little more effort of maybe one or two generations of scientists, the nature of all things will be as clear to us as an open book. Indeed, if this does happen, the old feeling of adventure will be taken away from scientific research, no more striking discoveries will be expected, and the task of scientists of the future will be reduced merely to a study of comparatively uninteresting minor details. Many scientists with whom I have discussed this question disagree with such a point of view, and to quote the French mathematician Laplace: "The larger the area which we have explored, the larger are the frontiers of the unknown." This is a very nice philosophical statement, but try to apply it to exploration of the earth. The geographical map of the ancient Greeks was very limited indeed and so were the contemporary frontiers formed by dark central Africa, mysterious Asia, and the cold expanses of northern Europe. The travels of Marco Polo, Columbus, Magellan, and other great explorers of the past, broadened the horizons of the Unknown, but very soon thereafter these horizons began to shrink. Nowadays navigators do not look forward to the discovery of new islands, not to mention new continents, and there is not much chance for the discovery of either hidden rivers or unmapped mountains. Thus, after the brilliant and exciting past, geographical exploration has finally come to a dull end.

Of course, the world of science is immensely broader than the world of geography. It involves studies extending to the distances of billions of light years out into space, studies of particles billions of times smaller than the tiny particles of dust, and the study of the living organisms which are billions of billions times more complicated than the most intricate electronic equipment. But, do we have a right to expect that this complexity of the universe has no end, and that the deeper we dig into the mysteries of nature the more new mysteries will appear ahead of us? Let us investigate the frontiers of the three leading sciences, physics, astronomy, and biology, and see what the situation is in respect to further horizons of exploration.

We will start with the problems of physics which involve the structure of matter. The ancient Greek philosophers suspected that all seemingly continuous material bodies were built from invisibly small, discrete particles which they called the "atoms"; *i.e.*, the indivisibles. They recognized four kinds of different atoms: those of stone, water, air, and fire. Wood was considered to be a combination of stone, water, and fire atoms, and the latter was liberated in the process of burning. Different metals were considered to be the combinations of stone and fire atoms mixed in various proportions. By adding more fire atoms to dull metals like iron or lead, it was supposed to be possible to produce shining gold. Although the details of that picture, which dominated medieval alchemy, were quite wrong, the idea of elementary substances was basically right, and now is forming the foundation of all modern chemistry. Later developments in chemistry have increased the number of elementary substances from only four to the ninety-two chemical elements forming the periodic system, and the structure of various chemical compounds has become much more complicated than it was first supposed by the ancient Greek philosophers. If the atoms had really been elementary particles, we would now have to deal with over a thousand different atomic species corresponding to the atoms of different elements and their isotopes.

The situation was, however, much simplified during the first two decades of the present century, when the study of radioactivity and the discovery of artificial transformation of elements led us to the conclusion that atoms are actually complex mechanical systems formed by only a few different kinds of constituent particles. For a while there were only two such constituent particles: heavy positively charged *protons*, and light negatively charged *electrons*, and the picture seemed to be very simple indeed. However, very soon the new particles, with the claim to elementarity, started to appear. First, in 1932 there appeared the *neutron* which was very similar to a proton except for the absence of electric charge. In fact, we now know that protons and neutrons can be transformed into one another through the emission, or absorption, of an electric charge. Protons and neutrons are now known under the collective name of

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nucleons. Along with neutrons appeared a hypothetical particle called *neutrino* which had neither charge nor any mass to speak of. Neutrinos are emitted by decaying atomic nuclei along with electrons and take up a part of the liberated nuclear energy. They possess tremendous penetrating power in their passage through matter and can cross the body of the earth from here to antipodes practically without any absorption. For thirty years since their original introduction into physics as hypothetical particles they managed to escape any detection, and only this year their existence was directly proved by a very ingenious experiment.

In 1933 the study of cosmic rays brought forth another new particle: a positively charged electron, known as an *antielectron*, or a *positron*. A positron has exactly the same physical properties as an ordinary electron, except for the opposite sign of its electric charge. When these two particles encounter each other, they are mutually annihilated with their mass being transformed into the energy of gamma rays emitted from the collision site. Ever since the discovery of positrons, physicists suspected the existence of *antiprotons* or *negative protons*, and special multibillion-volt electric accelerators were constructed for their detection. And, indeed, last year the existence of negative protons was confirmed by direct experiments carried out in California by means of an accelerator named "Bevatron."

Soon thereafter experiments also confirmed the existence of *antineutron*, which can be annihilated in a collision with an ordinary neutron with the entire mass of both particles being turned into radiant energy.

Next on the list of new particles are the so-called *mesons* with a mass intermediate between nucleons and electrons. As it was stated in the case of positrons, the existence of mesons was detected by the study of cosmic rays, and it was found that there are two principal brands of these particles: lighter mesons or *muons*, and heavier mesons or *pions*. Muons can be positively or negatively charged according to the idea of particles and antiparticles, while pions can also exist in a noncharged state. Further studies revealed the existence of several other meson brands and also the existence of particles heavier than nucleons which were called *hyperons*. Altogether we now have about two dozen different elementary particles!

Does this mean that nature is again making things more complicated for us? I do not think so. The point is that in most of these cases it was not an unexpected discovery of something the existence of which had not even been suspected, but rather an experimental confirmation of predictions based on the existing theoretical picture of the structure of matter.

The existence of "chargeless protons"; *i.e.*, neutrons, was suspected by nuclear theorists long before they were found experimentally.

The idea of *neutrino* was conceived by the Austrian theoretician, Wolfgang Pauli, and the neutrino remained a purely hypothetical particle until its existence was confirmed by a direct experiment almost a quarter of a century later.

The necessity of the existence of *antiparticles; i.e.*, positive electrons and negative protons, followed from a rather abstract mathematical theory developed in England by Paul Antoine Maurice Dirac. Their subsequent discovery represents a valuable confirmation of this theory.

Finally, the *mesons* were first introduced into science by the Japanese theoretical physicist, Hedeki Yukawa, as hypothetical particles necessary for understanding the forces which hold together atomic nuclei.

In all these instances it was not an unexpected discovery, as for example, in the case of the discovery of X rays by Roentgen and of radioactivity by Bequerelle, but rather it was an experimental confirmation of what had been expected.

The situation rather resembles that which existed in chemistry during the period after the periodic system of elements was proposed by Dimitri Mendeleev on the basis of theoretical considerations, when the vacant places in the periodic table were gradually filled up by subsequent experimental work.

True enough, the present state of the theory of elementary particles is far from being satisfactory and, in a way, resembles the state of the theory of atoms at the time of the formulation of the periodic system of chemical elements. We have still to understand why these particular particles, with these particular electric charges, masses, and other physical properties, exist in nature in preference to any other possibility. Very little progress has been made so far in this direction, and we are still facing a stone wall on our way toward the final solution. But, sooner or later, maybe tomorrow, maybe in a few decades, this barrier will be removed, and we will be able to understand all the observed properties of elementary particles on the basis of a simple physical theory. We will then know why neutrons decay into protons, why pions decay into muons, etc. What then?

Of course, there is always a possibility that the future theory of elementary particles will lead to the conclusion that they are not elementary at all, but are, in their turn, built up from still smaller "subelementary" particles, which will later turn out to be also composite, etc. It does not seem, however, that such a sequence of elementary, subelementary, sub-subelementary, sub-sub-subelementary, and so on . . . , particles is what is lying ahead of us. Rather it seems that what we now call elementary particles are really the ultimate structural units of matter.

I know that I can easily be accused of making the same mistake of many physicists of the last century, who believed that atoms were really and truly indivisible. But I think that the situation with respect to what we now call elementary particles is rather different from the case of the allegedly indivisible atoms. The atoms were known to have very complex properties: their optical spectra often contain thousands of individual lines,

they obey rather complex laws of chemical interactions, etc., etc. To assume that the particles, which show such a complex array of physical and chemical properties, are really elementary would be the same as assuming that a grand piano is an elementary musical instrument after listening to a piece of music played on it! On the other hand, elementary particles seem to possess very simple properties, and in this sense could be compared rather to tuning forks of several different shapes and sizes. And there is not much complexity in a tuning fork! Thus, it seems to me that, after the problem of elementary particles is finally understood, the heroic era of exploration in physics will terminate, and further research will be limited to a mopping up of already conquered grounds. Of course, there will always be something left to do concerning various innumerable details, but it will be more in the nature of the work of professional geographers of today, rather than the deeds of the great explorers of the past.

Let us turn now to the problems of astronomy. Here again we can remember the glorious path of early exploration. The sun, which was just a ball of fire floating in the atmosphere in the eyes of the ancient Greek, and a mere companion of the Earth in the Ptolemean system of the world, became the center of the planetary system due to the work of Copernicus. Subsequent development of astronomy has shown, however, that our sun, brilliant as it seems to us because of its short distance from the earth, is only an average member in the giant stellar family of the Milky Way, which contains about one hundred billion individual stars, many of them much brighter than the sun. The twentieth century witnessed a still further extension of astronomical horizons when, mostly due to the work of an American astronomer, Edwin Hubble, we learned that the space around our stellar system of the Milky Way is populated by a multitude of other similar stellar systems or Galaxies, some of which are even larger than our own. Thus, astronomical horizons, which were originally limited mostly by our own planetary system, were expanded a million fold by studies of the stars of the Milky Way, and a billion fold by the exploration of other Galaxies located far beyond the limits of our stellar system. The pre-Copernican universe, and the universe as we know it now stand much farther apart from each other than the ancient Greek geographical map and a geographical atlas of today. It seems, however, that the present astronomical picture of the universe is a finite picture and will not be essentially modified by further penetration into space. In fact, construction of the 100-inch telescope at Mount Wilson Observatory, which penetrated farther into space than the instruments previously used, has shown only more of the Galaxies located farther away. The 200-inch telescope at Palomar Mountain Observatory, reaching twice as far into space as the Mount Wilson instrument, shows that a similar situation exists up to the limit of its power, and the most recent studies, made by means of the so-called radiotelescopes, seem to indicate that the world of Galaxies extends far beyond the range of existing optical instruments. It seems, in fact, that the world of Galaxies extends all the way into infinity, and no matter how much we increase the range of our observations, we will still see essentially the same picture.

Please do not misunderstand me. I do not mean to say that the use of larger and better telescopes did not lead to any new discoveries. It certainly permitted a much better study of the structure of individual Galaxies, their distribution in space, and has recently led to the observation of an exciting phenomenon of a collision between the Galaxies. What I want to say is that the further exploration in space did not lead to any essential change of our views concerning the universe based on the previous more limited observations. In this sense, the general picture of the universe as an infinite space populated more or less uniformly by stellar Galaxies is here to stay! Just as in the case of the structure of matter, we seem to have now grasped the fundamental features of the structure of the entire universe.

Similar situations exist in the problem concerning the structure and evolution of individual stars which form the Galaxies and, in particular, in the problem of our own sun. Up to only several decades ago, the problem of energy sources of our sun and all other stars was quite obscure. However, the work of the famous English astronomer, Sir Arthur Eddington, on the internal structure of stars, and the more recent explanation of the almost limitless energy supply of the sun and the stars are due to the nuclear reactions taking place in their hot interior, which cleared the picture quite a bit. We now know for certain that our sun, as well as all other stars, receives its energy from a steady transformation of its original hydrogen supply into helium, and we can calculate in great detail the rates at which these transformations are taking place in different stars. We can follow in detail the successive stages of stellar evolution, which begins with the condensation of a young star from the cold and diffused interstellar gas, goes through a long period of steady shining, and finishes in a violent stellar explosion which scatters the material of the star far and wide through the surrounding space. These theoretical considerations stand in perfect agreement with what we actually see in the starry sky. In fact, due to the varying lifespan of the stars of different brightness, we observe the stars passing through various stages of their evolutionary life, and we can chart the life of a star from its babyhood to its death berth as easily as it can be done in the case of human beings. In particular, in the case of our sun we know that it, together with most of the stars, was formed from dilute gas about five billion years ago, and that, at the present time, it is just about halfway through its adult life. The sun will shine for another five billion years just as it shines today until the aging process begins. At that time the sun will begin to expand to a few hundred times its present size and will become redder and redder. In fact, we see many

stars in the sky, known as Red Giants, which are now passing through this stage of evolution. The expansion will be followed by ultimate contraction, finishing in a violent explosion which will scatter the material of the sun in all directions through space. Explosions of that type are observed from time to time among the stars of our stellar system—the Milky Way—and in other Galaxies, and they are known as Supernovae. Studying the explosions of other stars which have aged faster than our sun, we can form a very good picture of what the death of a star looks like.

I have given you this general picture of stellar evolution just to underline the fact that our present knowledge of the nature of stars is almost completed, and that today the astronomer knows the stellar mechanism almost as well as a garage mechanic knows the mechanism of an automobile. Here, as in other fields of science discussed before, further progress is bound to be limited by the study of only more or less important details without any drastic changes in the present picture of our universe.

We are coming now to the third important branch of the natural sciences: the study of living organisms, and the inquiry into the nature of life itself. Only recently, biologists were divided into two opposing groups: the "mechanists," who maintained that all the phenomena of life can be understood on the basis of the fundamental laws of physics and chemistry, and the "vitalists," who believed that all living processes involve an entirely different kind of force known as "vis vitalis." Our increased knowledge of the fundamental biological phenomena gained during recent decades removed the idea of "vis vitalis" into the realm of scientific phantasies, and I do not think that there exists a single research biologist who today would doubt that all the processes of life are based entirely on physico-chemical phenomena. The recent increase of our knowledge and understanding of the living processes is due, to a large extent, to the study of the simplest living organisms known as viruses. Since the great work of Louis Pasteur, and until only a few decades ago, it was believed that the smallest and simplest forms of life are represented by various bacteria, the microorganisms which measure only a few thousandths of a millimeter across. It was later discovered, however, that many diseases of animals and plants are caused by something that is much smaller than bacteria—something that can penetrate through the finest filters, and cannot be seen even through the best microscopes. Only the development of the so-called electron microscope, which uses beams of tiny electrons instead of ordinary light rays, opened our eyes to the real nature of these semimicroscopic organisms, generally known as viruses. Electromicrograms of viruses show that here we are dealing with organisms a hundred times smaller in size than ordinary bacteria. Some of them, as for example the virus causing influenza, are spherical in shape, while others, such as the virus causing the mosaic disease in the tobacco plant, have the shape of long rods. Since they are much smaller than bacteria, viruses can attack bacteria of different kinds by penetrating into their bodies and by multiplying inside them, in much the same way as the bacteria themselves multiply inside larger organisms. The viruses which attack bacteria are known as "bacteriophages" and often have unusual forms, such as polyhedrons with an attachment looking like a short tail. All viruses of a given type are identical to each other in shape and size, showing no variance between different individuals, such as we observe in the case of more complex organisms. In fact, it seems to be true that two viruses of the same kind are identical down to the last single atom forming their structure.

In the study of viruses, we apparently encounter a missing link between the organic and inorganic world. On one hand, their identity to each other in the chemical sense, and their ability to form crystals similar to the crystals of ordinary inorganic substances place them in the same class as ordinary molecules, such as those of water, salt, or sugar. On the other hand, viruses entering into the body of living cells begin to behave in very much the same way as the small microorganisms. After a single virus particle enters into the cytoplasm of a cell, it begins to multiply giving rise to a hundred or more identical particles, which ultimately break up the cell in which they were bred and attack the neighboring cells of the invaded organism. Similar to all other more complex organisms, viruses undergo mutations, with spontaneous changes in their properties, which are then perpetuated through generations and generations according to the well-known laws of inheritance. It was recently found that in the cases where a cell is infected simultaneously by two viruses with slightly different properties, the progeny formed in the multiplication process has the properties which represent a mixture of the two paternal viruses. In fact, the laws, which govern the heredity in the case of viruses, are exactly the same as those observed in the case of all higher organisms including man.

The study of viruses presents the biologist with a unique opportunity to study the phenomenon of life on what can be called a molecular level, when a living organism can be described by a well defined, though rather long, chemical formula. All changes in the organisms can be described exactly in terms of the motion of individual atoms which, of course, take place in strict accord with the basic laws of physics and chemistry. Thus we know, for example, that the long rods of tobacco mosaic virus consist of two chemically different types of components. The core of each rod is formed by a bunch of twenty-four long and thin molecules of the so-called nucleic acid. This core is surrounded by thread-like molecules of protein, which are coiled around it as wire is around the core of an electromagnet. All hereditary information of the virus particle, which permits it to produce hundreds of identical particles when multiplying within a cell of a tobacco plant, is carried by the

nucleic acid molecules of the core whereas the protein coil around it represents only a protective skin. When a virus particle attacks a living cell, the protein skin is left outside while the nucleic acid molecules enter the cytoplasm and begin to multiply, each giving rise to about a hundred identical molecules. When the multiplication process is finished, brand new protein skins are synthesized from the material of the invaded cell, and we have a new generation of viruses ready for further action. This is not the place to consider all details of the above sketched picture; its main features have been discussed in order to stress the point that, in the case of such simple organisms as viruses, the basic processes characteristic of a living being can be described unambiguously in terms of ordinary physicochemical phenomena.

In the case of such simple organisms as viruses it is not only possible to understand every single step in their life cycle on the basis of simple laws of physics and chemistry, but we are also on the verge of the possibility of synthesizing these organisms directly from simple inorganic compounds. In fact, in the fall of 1955 two American scientists, Dr. Frenkel-Conrad and Dr. Williams, announced that they were able to analyze the tobacco mosaic virus into its two simple chemical components and then to put it back together again. When nucleic acid molecules, forming the core of the virus, and protein molecules, forming its protective skin, are separated from one another, they lose all the properties associated with the notion of life and become just the ordinary chemical molecules. Water solutions of the separated nucleic acid and protein molecules kept in two separate bottles do not differ in any respect from the water solutions of any other organic compound, such as ordinary sugar or citric acid. However, when the contents of the two bottles are put together, protein molecules begin to wind around the molecules of nucleic acid forming regular rods identical with those of the live tobacco mosaic virus. Applying this newly formed virus to the leaves of the tobacco plant, one observes a regular infection process, and this synthetic virus begins to multiply, spreading from cell to cell in exactly the same way as the native virus. Of course, one can say that in this experiment the protein and the nucleic acid used in the solution were not synthesized from simple elements, but were extracted from the live virus particle of tobacco mosaic. It must be remembered, however, that biochemistry of today has developed methods for synthesizing comparatively simple protein and nucleic acid molecules from the elements, and that it is only a question of time before more complex molecules, as those encountered in viruses, will be produced artificially. Thus, in principle, the experiment of Frenkel-Conrad and Williams can truly be considered as an artifical production of the simplest form of life.

How far are we, then, from the artificial production of more complex organisms such as a mouse or even a man? Of course, there is an enormous span between a virus particle and a human being! But let us not forget that a human being, or any other complex organism, is nothing but a large and highly differentiated colony of individual cells which have all developed from a single protocell: the fertilized egg. Thus, we do not have to produce the legendary "homunculus" with the head, the hands, and the legs, but only a single ovum and a sperm. Recent studies indicate that the penetration of a sperm into an ovum in the process of fertilization is very similar to the infection of a cell by a virus particle. In both cases the heredity-carrying molecules of nucleic acid contained in the sperm, or in the virus particle, penetrate through the cell wall and try to organize the cytoplasm of the cell according to the instructions they carry. If the penetrated cell is a female sex cell or an ovum which contains in its nucleus only one set of chromosomes carrying the maternal heredity, the nucleic acid molecules forming these chromosomes and the invading nucleic acid molecules from the sperm work harmoniously together, and the fertilized ovum develops through successive divisions into one or the other kind of organism.

If, on the other hand, as it is in the case of virus infection of ordinary cells, the cellar nucleus already contains two sets of chromosomes with maternal and paternal hereditary information, there arises a strong conflict leading to the destruction of the cell. Thus, strictly speaking, in order to produce a synthetic complex organism one only has to synthesize the nucleic acid molecules contained in the sperm and in the nucleus of the ovum. However, this task should not be much more difficult than the synthesis of nucleic acid molecules forming the tobacco mosaic or any other virus. Thus, at least in principle, we now see the way of producing living organisms of any complexity from the simple chemical elements, even though the actual task of carrying out that program will undoubtedly encounter many serious technical difficulties.

Probably the last fortress standing in our way of complete understanding of life is presented by the problem of the functioning of the brain, which is undoubtedly the highest product of organic evolution. There exists an opinion that here we are bound to encounter a peculiar difficulty in trying to understand the functions of our brain by using the very same brain, but I do not think that this paradoxically sounding argument has any real significance. We must remember, however, that the structure and the functioning of the human brain belongs to the next higher echelon of biological problems: the sociology of individual cells which, in their symbiosis, form the complex cellar colonies. While the cells forming our body are subject to essentially the same basic laws as the simple monocellar organisms, the nervous system appears only in animals at a comparatively high degree of evolutionary development and is completely absent in plants. Due to the complexity of higher organisms, the detailed understanding of physiological processes taking place in their bodies becomes necessarily more complicated. Thus, for example, the problem

of muscle contraction, which may seem to be a comparatively simple one, is actually farther away from its solution than such an admittedly complicated problem as that of the storage and transfer of hereditary information. The situation is similar to that encountered in physics and chemistry where the nuclear processes taking place in the explosion of an atomic or a hydrogen bomb can be precalculated with great precision, while the chemical reactions in an ordinary gun powder or dynamite still form a highly disputable ground among chemists. Thus, it is likely that the solution of problems connected with the functioning of the brain will be one of the last problems solved by the biological studies.

On the basis of everything I have said, it really seems that in the exploration of the universe, the study of the innermost structure of matter, and in the understanding of the nature of life, the science of today approaches the bottom of the barrel of mysteries, and it is my earnest opinion that the twentieth century will play the same role in the history of our exploration of both macrocosm and microcosm, as the era of the great geographical discoveries played in the exploration of the surface of the earth. Maybe I am wrong, but I do not think so, or, at least, I will never know if I am.

"In the fall of 1943, Allied intelligence reports contained disturbing news. The indication was that German submarines were being fitted with a new and considerably improved radar. A high-performance set of this type could constitute a very serious threat to our own radar-equipped search planes which had driven the U-boats down and which had helped to break the back of Doenitz's undersea campaign. If the submarine could see an approaching plane, it would then have ample time in which to submerge to a safe depth. Our planes would still be able to force the subs down, but the enemy radar could remove the element of surprise and thus prevent our planes from making kills.

The Navy was very much interested in air-borne radar search equipment capable of operating in the frequency range understood to be used by the German sub radar. The new direction finder under development at an NDRC laboratory seemed to provide an ideal answer. It was felt that submarine radar transmissions might be intermittent; hence, any device which displayed the direction of a signal instantaneously on a cathode-ray tube in much the same manner as a radar displays its echoes seemed to fit the requirements perfectly. This the new direction finder could easily do.

In September, 1943, a laboratory prototype direction finder was installed aboard a Navy sea-search PB4Y-1 patrol bomber on the very highest priority. One month later, the PB4Y-1 left for a base in North Africa, from which place it flew over 500 hours in submarine searches over the U-boat hunting grounds in the Bay of Biscay and in the Mediterranean.

Throughout many long and tedious flights the equipment functioned perfectly—but no enemy signals were heard. Back home, anxiety over the supposed submarine radar menace subsided. It appeared that the new German radar was either no good or was not being used.

At the end of the war, the correctness of the latter conclusion was verified. Interrogations of captured submarine crews revealed that the U-boat skippers were downright afraid to turn on their equipment. By preference, the German radar was used only in regions close to the homeland, and then only for navigation in narrow passages. Out in the open ocean, it was almost never used.

This German fear of being overheard could be interpreted as a tribute to our radar countermeasures organization—a tribute which was perhaps not quite justified at the time. However, the fact remains that countermeasures—or the threat of countermeasures—prevented the German U-boat commanders from making use of their radar—a weapon which has made our own submarines many times more effective."

"Electronics Warfare—A Report on Radar Countermeasures"
Joint Board on Scientific Information Policy, Washington, D. C., 1945

Contributors

George Gamow, one of the world's foremost scientists in the field of theoretical physics, was born in Odessa, Russia, on March 4, 1904. He received the Ph.D. degree in physics from the University of Leningrad in 1929, and attended the University of Copenhagen as a Fellow of the Theoretical Physics Institute in 1928 and 1929. In 1929–1930 he was a Fellow of Rockefellow Foundation at Cambridge, England, and returned to the University of Copenhagen on a fellowship again from 1930–1931. He was a professor at the University of Leningrad from 1931 to 1933, and the following year was a Fellow of Pierre Curie Institute in Paris. From 1934 to 1956 he was a professor of theoretical physics at George Washington University. At present, Dr. Gamow is professor of physics at the University of Colorado.

G. Gamow

Dr. Gamow has done consulting work for various organizations on a part-time basis. He was a member of the Air Force Scientific Advisory Board, and a visiting professor at the University of California.

Among his contributions to science are the Gamow theory of radioactive decay, the origin of chemical elements, and the theory of protein's synthesis. He is a member of the National Academy of Sciences, the Royal Danish Academy of Sciences, the American Geophysical Society, the International Astronomical Union, and many others.

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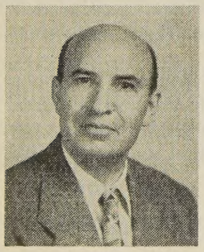
Hugh Gray Lieber was born on April 17, 1896 in Maryville, Mo. He served as Captain in the Coast Artillery from 1917–1919. He received the B.A. degree from the University of Oklahoma in 1919, and the M.A. degree from Columbia University in 1923.



H. G. Lieber

Mr. Lieber has done many drawings and paintings, including the illustrations for Theodore Dreiser's "Moods," and for books on modern mathematics by Lillian Lieber. He has taught mathematics for many years, and has been professor and Head of the Department of Fine Arts at Long Island University since 1945. He has been art consultant at the Galois Institute of Mathematics and Art, Brooklyn, N. Y., since 1932. His publications of abstract drawings include "Goodbye, Mr. Man, Hello Mr. NEWman," "Comedie Internationale," and a book of verse and drawings.

He is a member of the Museum of Modern Art and Phi Beta Kappa.

Lillian R. Lieber was born on July 26, 1886 in Russia. She received the B.A. degree from Barnard College in 1908, the M.A. degree from Columbia University in 1911, and the Ph.D. degree from Clark University in 1914. She was Head of the Physics Department at Wells College in 1917–1918, and held the same position at Connecticut College for Women from 1918 to 1920. From 1945 to 1954 she was professor and Head of the Mathematics Department at Long Island University. Dr. Lieber is the author of "Non-Euclidean Geometry," "Galois and the Theory of Groups," "The Einstein Theory of Relativity," "The Education of T. C. Mits," "Mits, Wits, and Logic," "Infinity," and "Human Values of Modern Mathematics."

L. R. Lieber

She founded the Galois Institute of Mathematics and Art, and has been its Director since 1932. She is a Fellow of the American Association for the Advancement of Science, and a member of the American Mathematical Society and the Mathematics Association of America.

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Philip McCord Morse was born in Shreveport, La., on August 6, 1903. He received the B.S. degree in 1926 from Case Institute, which awarded him an honorary Doctor of Science degree in 1940. He attended Princeton University on a fellowship from 1926–1928, and received the A.M. degree in 1927. The following year he was Jacobus Fellow, and received the Ph.D. degree in physics in 1929. The next two years were spent at the Institute of Physics at Princeton, and in Munich and Cambridge as an International Research Fellow. Since 1931, Dr. Morse has been with the department of physics of Massachusetts Institute of Technology, where he is now a full professor. He was director of the underwater sound laboratory at M.I.T. from 1939 to 1942, and supervisor of the sound control laboratory at Harvard University from 1939 to 1945. He also was director of Brookhaven National Laboratory from 1946 to 1948.

P. M. Morse

Dr. Morse was associated with the Office of Scientific Research and Development of the Navy from 1942 to 1946, where he directed an operations research group. He also served as deputy director of a weapons systems evaluation group for the Office of the Secretary of Defense from 1949 to 1950.

His achievements have been in the fields of quantum theory, nuclear theory, acoustics, theory of stellar structure, electric discharge in gases, electromagnetic theory, and operations research.

He is a Fellow of the Acoustical Society of America, the Physical Society, the American Academy, and the Operations Research Society, and a member of the Scientific Research Society and the New York Academy.

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Sir Robert Watson-Watt (SM'46–F'47), the man who gave Great Britain's Defense Forces their magic eye, was presented £50,000 by the Royal Commission on Awards to Inventors for his "initiation of radar and his contribution to the development of radar installations," before and during World War II. This was the largest sum ever awarded by the Commission to an individual. It is less well known that he is also the inventor of the instantaneous visual radio direction-finder which the historian of U.S. Naval Operations in World War II places no lower than radar in its contribution to victory in the anti-U-boat war, the Battle of the Atlantic.

R. Watson-Watt

Sir Robert was born in Scotland on April 13, 1892. He graduated in electrical engineering at the University of St. Andrews in 1912, and immediately became assistant to the professor of natural philosophy in University College, Dundee. Appointed to the British Weather Service, he became meteorologist-in-charge of the Branch Meteorological Office at the Royal Aircraft Establishment in 1917.

In 1934, when the work of Radio Research Station was merged with the radio program of the National Physical Laboratory, Sir Robert was appointed the first superintendent of the Radio Department of the Laboratory. He has held many notable government positions in the capacity of scientific advisor and consultant, including the Deputy Chairmanship of the Radio Board of the War Cabinet.

His honors include his knighthood, conferred in 1942, the Companionship of the Order of the Bath, awarded in 1941, Fellowship of the Royal Society, the U.S. Medal for Merit, the Valdemar Poulsen Gold Medal of the Danish Academy of the Technical Sciences, and the Hughes Medal of the Royal Society for his pioneer researches in radio-telegraphy.

He is now a Canadian resident, and is president of scientific advisory companies in London, New York, and Montreal.

Sir Robert was 1950 IRE President, and also served as Director at that time. He is the president of the Royal Meteorological Society, and the Institute of Navigation, and is a past chairman of the Radio Section of the Institution of Electrical Engineers.

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